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ANNALS  
OF  
PHILOSOPHY.

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ANNALS  
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JANUARY, 1825.

ARTICLE I.

*On the Life and Writings of Claude-Louis Berthollet.*  
By Mr. Hugh Colquhoun.

THERE are some men whose characters combine those estimable qualities which render them the delight of their friends, with those splendid talents which destine them to form an era in that branch of study to which they devote themselves,—men, whose memories should live from age to age endeared to the cultivators of science, a generous incitement to their ardour as students, and a bright example to their conduct as philosophers. Such a friend, and such a man of genius, was the subject of this memoir; nor needs there much of prophecy to pronounce that such also shall long be the hallowed memory of Claude-Louis Berthollet.

He was a man, whose thirst after science was strong in his earliest youth, and remained unabated during the extended period of a busy half century. In all this time, neither the perplexing subversion of the old system of his favourite study could damp his zeal, nor the revolution in the government of his country withdraw his attention from the constant pursuit of chemistry. And it surely yields one a pleasure of no ordinary kind to reflect, that during the frightful tempests which agitated the political world throughout the life of this child of science, we find the sphere of his pursuits to have been placed beyond the reach of the storm; nor can a greater contrast be imagined than the even tenour of his useful life presents to all the baneful changes and desolating wars that meantime oppressed his country and the world.

During the long life which Berthollet thus devoted to science, he is uniformly found with a pure and disinterested ardour of research, pressing on from discovery to discovery, and using each new step that he gained, as an instrument of farther and more powerful research into the hidden relations of nature. Independent in his opinions, he frequently stands alone in doubting, or at least in qualifying the most prevalent dogmas of

the day, and these doubts have been changed by subsequent discovery into certain objections against those theories, now that their merits are discussed with more cool discrimination. We must not, however, suppose that Berthollet is always as correct as he is original, or that his views are as unerring as they are profound. On the contrary, he is not only wrong sometimes, but occasionally a little obstinate in his prejudices. In return for this, however, we find him openly and manfully renouncing his adherence to an erroneous opinion the moment that full conviction has forced itself upon his understanding. And if, in some cases, his errors were of a longer duration, we need not, therefore, be surprised, since the amazing ingenuity of his experiments and of his reasoning has oftener than once, in such circumstances, compelled the whole world of science, for twenty years together, to yield implicit assent to his doctrines. Nor was one of his peculiar and most characteristic features the least honourable to himself, or the least useful to his fellow men. For he was not one of those profound theoretical speculators, who, in the energy of their abstraction, forget the practical applications of which their discoveries are susceptible. Far from this, Berthollet, while he loved science for itself, also loved to teach it how to foster the arts. On one occasion in particular, he was so eminently original and successful in the substance he employed, and the method he pointed out, for improving one of the most useful arts, that his name was given to his system, and by the common sanction of his countrymen, to perform this process, was called *berthollet*, the workman *bertholleur*, and the manufactory *bertholleterie*. So that thus, if every other memorial were to perish, his name would nevertheless be familiar to all his succeeding countrymen, while the French language continues to be a spoken tongue.

Berthollet was not a native of France. That country claims him along with Cassini, and Winslow, and La Grange; says Cuvier,\* in the Eloge of which Berthollet is the subject, only as the son of her adoption, and whom it was her glory to foster and to cherish. He was born at the family mansion in Talloire, near Annecy, in Savoy, on the 9th of Dec. 1748. From this spot, he made his first progress into the world, to commence his studies at Chambéry, in prosecution of which he next proceeded to the Collège des Provinces at Turin, a celebrated establishment instituted by Charles Emmanuel III. King of Piedmont, where many of the distinguished men of talent which that coun-

\* The eloquent Eloge Historique de M. le Comte Berthollet, par M. le Baron Cuvier, which, as perpetual Secretary, he read to the Royal Institute of Paris in June last, is now before the world: I take this opportunity of paying my tribute to the elegance of that Eloge, and of adding, that I have not scrupled, in preparing the materials for this biography, to use it freely, whenever other sources seemed either defective, or but ill-authenticated.

try has produced, have been imbued with their first thirst for science.

Here the young Berthollet attached himself to the study of medicine, less, it may be supposed, from any views of interest to be gratified in its pursuit, than from that inclination already powerful, which soon became the master passion of his breast, for the investigation of those sciences which form the basis of the school of Hippocrates. He remained no longer at Turin than just to take the degrees in his profession, after which he proceeded to Paris, as the future theatre of his speculations and pursuits.

His first appearance in that capital was a singular one, and the first acquaintance he made is a remarkable proof of the open frankness of an honest and independent heart. In that immense city, Berthollet had not one friend; he had not even a single introduction to any one. But, at that time, it happened, that one of the most distinguished of the medical profession was Tronchin, a native of Geneva; and the young Savoyard conceived that in Paris he might be claimed as more than half a countryman. On this slender ground of introduction he waited upon Tronchin, and quite contrary to what the manners of the times might have led us to expect, his new-made acquaintance, prepossessed at first by his frankness and intelligence, grew gradually more and more attached to him, until intimacy ripened into firm friendship. Nor did this friend content himself with mere professions of regard, but soon, by means of his all-powerful influence with the Duke of Orleans, Louis, grandfather of the present Duke, and then uncle of the reigning king, he procured for his protégé the situation of one of the physicians in ordinary to that prince. In this situation, the independent character of the man, and his attachment to science, appeared. For while others found their way to rank and riches by their assiduity at Court, Berthollet at once and entirely abandoned himself to the prosecution of those studies, which continued to occupy and engross his whole after life. Let us endeavour to accompany him in his researches by detailing the principal discoveries that he made, by stating the various opinions that he maintained, and by describing the chief works that he published, whilst we occasionally survey the state of science in Europe at the era of each.

The first essays of M. Berthollet, and his first appearance as a philosopher, are so intimately connected with the revolution which the science of chemistry was then undergoing, that it is impossible to understand the one, or to appreciate the other, without a short view of the leading principles of the old and new systems. Nor ought we to forget, when we find our chemist somewhat obstinately wedded even to the absurdities of the old school, the length of time during which it had ruled without

dispute, and the number of illustrious names which it enrolled among its disciples. Analogy will suggest to every one that the same phenomena have accompanied each successive revolution in science, or in philosophy, or in religion, from the dawn of letters in the middle ages down to the present day.

The radical evil of the ancient system of chemistry, whose baneful influence pervaded every part of it, was Stahl's doctrine of *phlogiston*. When a metal is calcined under contact with the air, it is gradually converted into an incoherent earthy mass, formerly styled a *calx*. This *calx*, according to the old school, is itself a simple substance; and the metal is a compound of the *calx* and *phlogiston*. When a metal, therefore, is calcined, it, in their language, is resolved into the *calx*, its basis, and at the same time it loses some other thing unknown,—the ideal principle named *phlogiston*. To this hypothesis, the processes of experimenting, as they improved, furnished an insuperable objection. When a metal is converted into a *calx*, or gets rid of part of its composition, viz. *phlogiston*, it *increases considerably in weight*; and, on the contrary, when a *calx* is brought back to the metallic state, when it gains its *phlogistic* constituent, it *loses precisely the amount of weight which it had previously gained*. That is to say, the simple basis, the *calx*, is heavier than when to this same basis there is superadded *phlogiston*. To any unprepossessed mind, this objection is fatal to the hypothesis of Stahl; but men, bred up in any scientific creed, are not so easily induced to renounce their first belief. And, accordingly, the disciples of *Phlogiston* only declared that this substance is *specifically light*, or has a principle of levity; or to speak more clearly, that it paralyses the action of gravity.

However, the science of chemistry continued to advance, and her busy votaries, in every quarter of Europe, by the ardour of their researches, were every day making new and interesting experiments, the results of which circulated among them with electric rapidity. It is plain that in such a state of things, any theory, which every day put to the test, if radically vicious, must, notwithstanding its weight or prevalence, have its errors at length exposed; and after a struggle, perhaps severe, be utterly overthrown, and for ever discarded. Accordingly, whilst every other chemist in Europe, with an obsequiousness unfortunately more to be lamented than wondered at, was perplexing his judgment, and even distorting fact itself, in order to adapt the *phlogistic* theory to the progress of science, Lavoisier felt it every day more and more impossible to admit its accuracy. The important discoveries of Black, Priestley, Scheele, Cavendish, and others, respecting factitious airs, and the phenomena attendant on the calcination of metals, at an early period seemed to him, not corrective but subversive of the system of Stahl. And the process of reasoning by which he gradually arrived at his results is at

once so simple and so conclusive, that one cannot avoid wondering, with Cuvier, at the modest style which he assumed in arguing in support of the Antiphlogistic Theory, on the one hand, and at the confident tone of the obstinate phlogistians on the other. Lavoisier reasoned nearly as follows :

A metal calcined invariably gains a considerable increase of weight. In any given close vessel, only a determinate portion of metal can be calcined. Heat may be applied to the vessel in every various degree, and for any length of time: the quantity of metal which may be calcined within it has nevertheless its fixed limits, and calcination in such a vessel, once brought to a period, can never again be renewed. But if the vessel be now opened for a short time, and a fresh supply of atmospheric air admitted, the process of calcination may be renewed, and again carried on, but within the same limits as before. In the open air, metals may be calcined to any extent. After calcination in a close vessel, the body of air originally included has lost considerably in volume and weight, and has changed several of its properties. The increase of weight gained by the metal measures the exact loss of weight sustained by the air, so that the weight of the whole remains unaltered. From these premises, Lavoisier concluded, that since the presence of atmospheric air is essential to calcination, since a given quantity of air serves to calcine only a given quantity of metal, and since this process invariably transfers a given weight from the air to the metal, calcination must consist in the absorption of a ponderable principle from the air.

Surely no process of reasoning could be more simple—no results seem more inevitable than these; and just at this time an experiment made by Dr. Priestley enabled Lavoisier to give an analytical demonstration of his theory.

When mercury is calcined in a close vessel, it is gradually converted into a red coloured calx: at the same time a portion of the confined air disappears, and the residue is incapable of contributing to new calcination, or of maintaining either combustion or respiration. If the red calx be now exposed to a stronger heat in contact with this deteriorated air, the metal and the air simultaneously assume their original appearance, and recover their original properties. The phenomena of this experiment at once furnished Lavoisier with the analytical and synthetical tests of his theory, and enabled him to prove that atmospheric air is no element, but a compound substance, of which one constituent can support combustion and respiration, while the other cannot.

He next generalized the subject by showing that in all combustions, a portion of the atmospheric air combines with the combustible.

There still remained one serious deficit in the proofs of the truth of this theory. This arose from a phenomenon attending

the solution of metals in acids :—whence results so considerable a quantity of inflammable air? If the sole constituents of sulphuric acid be sulphur and oxygen, whence comes it that when it is brought into contact with a metal, with the addition of a little water, so large a quantity of inflammable air should be produced during their reaction? This objection, which at first appeared unanswerable, was soon converted into a proof of that theory which it threatened to subvert, by Cavendish's great discovery of the composition of water. He proved it to be no longer an element, but formed it by combining its constituents, oxygen and inflammable air. This experiment was eagerly laid hold of by Lavoisier, and repeated by him and his associates in 1783. And now Lavoisier's theory was established by an unbroken chain of reasoning from experiment, connecting the double processes of synthesis and analysis in its support, such as should have constrained all enlightened chemists to renounce for ever the ancient system of error.

This, however, was far from being the case : and the sketch which has just been given of the fundamental principles of the old and new systems of chemistry is necessary on two accounts in a life of Berthollet. It is necessary in the first place to understand the errors under which he laboured while yet he remained a staunch adherent of the theory of Stahl ; and it is so in the second, to explain the large share which his subsequent reasonings and discoveries had in elucidating and supporting the theory of Lavoisier, after he became fairly convinced of its truth.

The first extant memoir of Berthollet (which appeared in the *Journal de Physique* for 1776), the subject of which is Tartarous Acid, seems never to have been laid before the Academy of Sciences. The first which our chemist appears to have submitted to that learned body, is an essay on Sulphurous Acid, read in the end of the following year. It is the custom of the Academy, it may be here remarked, upon receiving any original memoir, to appoint one or more of their members to examine into its merits, and to report on them. Lavoisier was not unfrequently one of those who reported on Berthollet's earliest memoirs, and they all furnish most striking proofs at once of the extreme repugnance of the latter to adopt the doctrines of the new theory, even when these seemed most necessary to him, and of the great respect which the former showed even for the errors of our chemist, whose genius from the first he fondly and tenderly cherished. In this memoir on Sulphurous Acid, while Berthollet is compelled to admit that sulphur during its combustion unites with a portion of atmospheric air, he nevertheless, in viewing its constitution, most wantonly encumbers and perplexes his explanations with an unsparing use of the phlogiston of Stahl. Lavoisier regarded sulphur as a simple body, sulphurous acid as a compound of that body with a certain dose of

oxygen, and sulphuric acid as the same base united to a greater proportion of the same air. Berthollet, on the other hand, in his view of the constitution of sulphur and its two acids, gives a striking specimen of the old school of error driven to extremity, and unable either to check the progress of experiment and knowledge, or to go on with it. Sulphur, says Berthollet, is not a simple body, but a compound one, and its constituents are phlogiston and a base; sulphuric acid is a compound of phlogiston, the same base, and vital air or oxygen gas; and sulphurous acid is the same base united to less vital air than exists in sulphuric acid, and to less phlogiston than is found in sulphur. If this complex explanation be deprived of the phlogiston, with so large a dose of which it is combined, the exposition of the nature of sulphur and its acids given by Berthollet is not really different from that of Lavoisier. At the same time it is difficult to conceive how that chemist could preserve his patience at seeing theories, otherwise so excellent, wholly spoiled, and talents which might have been so usefully exerted, wholly frittered away by the bigotted support of a system which every day's experience made less and less defensible, and in defiance of a simple yet just doctrine, of which he had several years before developed the outlines, and had now nearly completed the proofs. Yet, at this time, he stood single in the Academy, and even Berthollet, while he admits the Lavoisierian principle of the presence of oxygen in these acids, cannot rest satisfied until he confuses and perplexes every thing by superinducing the erroneous views of Stahl upon the plainest facts and the simplest theory.

It seems surprising too, that a man who thought so freely for himself as Berthollet's whole after life proves him to have done, should so long have remained attached to the ill-founded system of Phlogiston. Yet, independent of the force of prejudice, which, once deep-seated, rules with most power the strongest minds, it is no more than justice to Berthollet to state, that he himself, in a memoir, read to the Academy in the beginning of 1778, on the subject of Sulphuretted Hydrogen Gas, details the experiments which became the foundation of a subsequent material restriction of the theory of Lavoisier. Of course, even if the conjecture of Cuvier be correct, that neither Berthollet nor Lavoisier at that time saw all the consequences resulting from this experiment; yet as the former chemist, in a few years after, resumed the subject, and was the first, by many a year, to lay down this very limitation of the doctrine of the latter, it is fair to suppose that, even at this time, he must almost *à son insçu*, have felt a powerful, and in this case a well-grounded prejudice, against a leading part of the new system.

It was unfortunately laid down by Lavoisier, as one of his fundamental principles, that oxygen constitutes the sole princi-

ple of acidification. In his memoir Berthollet shows that sulphuretted hydrogen gas, in which oxygen is not present, nevertheless performs *all the functions of an acid*: and surely it seems reasonable that a doctrine opposed *in toto* by every one, should not first be received as generally correct by him who alone had discovered any just grounds for qualifying one of its leading principles. Yet it is strange enough that this very man proved eventually the first leading chemist who did admit the just doctrines of the new theory, and it seems stranger still that those who held out longest against *its truths*, were also the first to embrace and defend *its errors*. But so it was; for Berthollet's subsequent assertion, arguments, and numerous decisive experiments, all proving oxygen not to constitute the sole principle of acidification, fell for many a year unheeded on the ears and understandings of men of science, until the united force of the facts brought forward by Gay-Lussac, Thenard, and Ampère, joined to the profound and admirable reasoning of Sir H. Davy, at length established the accuracy of this limitation and qualification of the principles laid down by Lavoisier.

In another memoir of our chemist, on the Nature of the Volatile Alkali, presented soon after to the Academy, he announced a theory of his own upon the subject, which proceeded upon a basis altogether erroneous. This essay was entrusted to Lavoisier, to report upon its merits to the Academy, who, with disinterested tenderness for the honour of his antagonist, dissuaded him from committing himself by the publication of his system; and Berthollet's conduct is not less to be admired for the assent which he immediately yielded to the kindness and to the experience of his adviser. The memoir was not published. His reputation was thus not publicly staked in support of any erroneous system; and the stimulus which this very restraint gave to the ardour of his researches, led him a few years afterwards to one of his most elegant discoveries, that of the true nature of the volatile alkali. It is impossible not to esteem so much generous co-operation on the part of these two illustrious chemists, eager only for the advancement of science, and opposed as they then were in many of their views; yet the younger remaining as free from distrust of his antagonist's advice, as the elder was untainted by jealousy of his rival's reputation.

In the subsequent experiments of Berthollet on the decomposition of nitre, phenomena presented themselves so easy an explanation on the antiphlogistic system, that it seems astonishing how even, prejudice itself could avoid the discovery of the true composition of nitric acid. At this time, however, Berthollet was prepossessed against the truth, he clung to the old system, and was rewarded accordingly: for the fine discovery that oxygen and azote are the constituents of nitric acid was thereby reserved for Cavendish.

It is unnecessary, however, to detail all the separate difficulties in which Berthollet was involved in common with the highest intellects of his day, from the same cause, that of having the mind previously warped by prejudice. Never was there a system which can bear the test of cool unprejudiced examination less than Stahl's theory of Phlogiston. That Proteus-principle, which performed the most inconsistent and contradictory functions; sometimes possessed of weight, tangible, and easily confinable by the simplest mechanical means; at other times, imponderable, invisible, and eluding all the efforts of the chemist to confine it within the compactest vessels; at other times, possessing even a principle of levity;—the chemical faith of the times, sat enthroned on the understandings of all men of science. And though nothing was more simple than Lavoisier's whole process of reasoning, while no result could be more inevitable than his, the leading doctrines of his theory had been propounded in 1773, and their proofs were nearly complete in 1777; yet they gained no adherent of any note until so late as 1785, when Berthollet became a convert to the truth of the system. So long previous to this, however, as 1777, we have seen him obliged to admit in his memoir on Sulphurous Acid, which was afterwards printed in 1782, that sulphur unites with oxygen during its combustion and acidification, and that it is heavier in consequence of it. And in another memoir, printed in the same year, in his "Researches on the Augmentation of Weight which Sulphur, Phosphorus, and Arsenic sustain, when they are converted into Acids," he employs the same doctrine. In this latter essay too, he expressly confirms the observation of Lavoisier, that any given volume of air is diminished during combustion to an extent, the weight of which is precisely gained by the combustible. It is in a memoir read by him in 1785, on the subject of Oxygenized Muriatic Acid, that he made a full and manly confession of the change which had taken place in his opinions, and in that very memoir combats Guyton de Morveau, one of the most illustrious disciples of the phlogistic school.

Previous to this time, however, M. Berthollet had given to the world several works, all of the highest scientific merit, and some at the same time of great practical value. Thus he was the first person who took an accurate view of the constitution of soaps, in his essay published in 1780, on the Combination of Oils with Alkalies, Earths, and Metallic Oxides. He therein showed that soaps are true chemical compounds, analogous in their nature to salts, and in which the oily principle performs the part of an acid. He also showed that this principle is capable of forming soaps, not merely by combining with the fixed alkalies, potash and soda, but also with the volatile alkali, with the alkaline earths, with the earths proper, with the metallic

oxides, and in short with every substance which, in combination with the stronger acids, forms a salt.

In the same year he published two memoirs, one on the Nature of Animal Substances, a subject which he more fully elucidated afterwards on the occasion of his brilliant discovery of the composition of ammonia; the other, on Phosphoric Acid, in which he succeeded in proving, that this acid exists ready formed in the animal body, and that it is not a product of putrefaction, or of the artificial processes employed to separate it, as was believed by some of the most eminent chemists of the day.

In 1781 he was elected Member of the Academy of Sciences at Paris, in preference to the celebrated Fourcroy, Quatremère d'Isjonval, and other competitors. This was one of the most distinguished learned bodies of which he could be chosen a member; and long previous to the close of his life, he had been elected into almost all the celebrated scientific societies in Europe, who were proud to enrol such a name as that of Berthollet among their fellows.

In the year 1784, M. Berthollet again found a competitor in M. Fourcroy, though the result was a different one. The death of Macquer left the chemical chair at the Jardin du Roi vacant, and M. Buffon, Intendant of that Institution, bestowed it on Fourcroy in preference to Berthollet. It is said that Buffon's vanity was piqued by the idea that the Duke of Orleans, who supported Berthollet's interest, had not paid him sufficient court on the occasion; but we may well say with Cuvier, that there is no need to recur to such a motive for the explanation of the ill success of Berthollet. For if his chemical acquirements and originality of thought procured him the seat in the Academy before Fourcroy, the fascinating elocution of the latter equally entitled him to be preferred to the professorial chair, which immediately under his auspices, engrossed the attention of crowds of admiring pupils.

Let us not here, however, omit to mention, that one of the situations which had been held by Macquer was at this time conferred on Berthollet. He was now appointed Government Commissary and Superintendent of the Dyeing Processes; and it may be supposed that this nomination necessarily turned his peculiar attention to the study of that useful art, into which he by and by introduced so many capital improvements.

The next memoir published by our chemist appeared two years after this, on the occasion of his succeeding in discovering the mode of obtaining the caustic fixed alkalies in a state of complete purity. This discovery, although not one of the least useful, is certainly not one of the most brilliant of those made by Berthollet, and indeed is chiefly remarkable as a proof that

even at this early period he stood pre-eminent among the chemists of his day, by his superior acquaintance with the resources of analysis, and by his greater penetration in foreseeing the new applications of which they were susceptible. But although this is not one of the discoveries which redounds most to the fame of the individual, it is one which has contributed most materially to the advancement of science. The pure caustic alkali has continued ever since that moment a most powerful instrument in the conduct of almost every department of analysis, in the animal, the vegetable, or the mineral kingdom; and to it we are especially indebted for almost all the knowledge we possess respecting the constitution of the precious stones, and other refractory mineral compounds. The greatest eclat does not always attend the most useful improvements.

The year 1785 was on many accounts a remarkable one in the life of Berthollet. In it he had the honour of being the first French chemist of any note who acceded to the doctrines of Lavoisier: in it he gave to the world his brilliant discovery of the composition of ammonia; and in the course of the same year, he published his first essay on the Nature of Dephlogisticated Marine Acid, or Chlorine, thus entering upon a field from which he afterwards reaped so rich a harvest of fame.

The constitution of azote and its combinations had long been a bar to the progress of the Lavoisierian doctrines. Nothing can be more strongly marked than the difference which exists between the natures of animal and vegetable substance, yet there was no subject whose investigation proved more difficult for chemists, than the cause of these distinctions. One of the first steps towards distinguishing these characteristics was made by Berthollet, when, in 1780, he showed that a large proportion of azote forms an invariable constituent in every animal substance. Still, however, the prominent part which azote performs in chemistry organic and inorganic, long continued an impenetrable mystery, and remained one of the last and most serious obstacles to the establishment of Lavoisier's theory. Nor need this mystery be wondered at, for at this time neither the composition of ammonia nor of nitric acid was known, and water, which so often mingled itself in every analysis, was yet regarded as an element.

The destructive distillation, or the spontaneous putrefaction of animal substances, gives invariably as one product a quantity of the volatile alkali: the same process applied to a vegetable principle, as certainly produces a substance of an acid nature. Bodies belonging to either class, when abandoned to spontaneous decomposition, yield matter which is eminently adapted to the support of vegetable life; but in addition to this, subjects of the animal kingdom, under certain circumstances, are charac-

terized by generating a great quantity of nitric acid, during the progress of decomposition. In what state of combination, it was vainly asked, do these three singular products, azote, ammonia, and nitric acid, or their constituents, exist in the animal body? It has been already remarked that Berthollet proved azote to be an invariable constituent of animal matter: he now proceeded a step farther by making the famous discovery that ammonia is a compound of azote and hydrogen. The only blank remaining to be filled up, with a view to the complete development of animal nature, was the exploring of the nature of nitric acid, which was successfully performed by Berthollet's friend, Cavendish, who showed it to consist of oxygen and azote.\* Berthollet was now enabled to form a completely new, simple, and satisfactory theory of the constitution of animal substance, founded entirely on experiment, and accounting easily for every appearance which had hitherto embarrassed the chemist. Animal substances, said he, differ from vegetable, by containing a large proportion of azote as an invariable constituent. During destructive distillation, or during putrefaction, the elements of the complex animal principles are disunited, and in obedience to the new affinities which are thus called into action, unite in new proportions, and form with each other more simple combinations. The azote, at this time disengaged, has a strong tendency to unite with the hydrogen (another invariable constituent of animal substance), the instant it is set free, and the product is ammonia. In a situation favourable to the union of the azote with oxygen, there will also be a formation of nitric acid.

Nothing could be more simple—nothing more complete, than this explanation; and by combining with it the brilliant discovery made shortly before by Cavendish, that water is a compound of oxygen and hydrogen, a lustre was shed abroad upon the science in every quarter, illuminating even those regions over which obscurity had previously hung her deepest shade. In almost every department of chemistry, there had till then been a number of important facts unexplained, and seemingly isolated, but which the intimate relations subsisting between the composition of these three substances served at once to elucidate and to connect. Chemistry, at this period, was at that stage of advancement, when an immense mass of facts had been accumulated, which, however, had no apparent dependence on each other, but which only required the regard of a master spirit to be thrown over them in order at once to appreciate their individual value, and their mutual relations, to penetrate the general and uniform laws and principles which govern them all, and to

\* So simultaneous were these important discoveries in the neighbouring kingdoms, that the private letters of the emulous friends, mutually announcing the discovery of each, are said to have actually passed each other on the way.

combine them into a simple and well-digested whole. This undertaking was made practicable after these discoveries of Berthollet and Cavendish, and the mode in which Lavoisier and Berthollet performed it ranks them among the first philosophers of the age.

As Berthollet was by this time confessedly one of the very first chemists of France, he almost necessarily became one of those who now undertook to introduce an important reform into the language of that science of which they had completely changed the system. Lavoisier, Berthollet, Fourcroy, and Guyton de Morveau, combined to plan and organise a new philosophical chemical nomenclature. Such an undertaking had long been a great desideratum, of which every day's experience made the necessity more pressing and imperious. After the important discoveries which had been made, and the many new views which had been introduced into the science, it became a matter of very great difficulty to describe the one or to explain the other in a language which had a constant reference to the phlogistic system. For Lavoisier and his confederates, this was wholly impossible, since the basis of the new system rested on the subversion of the old. They accordingly set about a radical reform where no palliative measures could be available, and if, after all the changes they effected, and all the improvements they introduced, by their "Methodical Nomenclature," there should still be discovered not a few omissions and anomalies, any feeling of regret that they did not do more should be absorbed in the gratitude that is justly due to them for having done so much.

Indeed it would be difficult to point out how even men so gifted as they were could have employed their talents in a manner more beneficial to science, than in the construction of this new language. The imagination can hardly conceive a more barbarous, repulsive, unmeaning chaos, than the chemical nomenclature had for more than a century presented. It was founded by Stahl in 1720, and it is easy to suppose how little the first attempt at methodising chemical facts, made in the very infancy of the science, would suit the rapid progress of discovery which characterised the 18th century. It retained not a few of the unintelligible terms of the alchemist, and moreover was adapted to the system of Phlogiston, so as to be wholly void of meaning when detached from it. Thus the access to knowledge was rendered unnecessarily thorny and difficult, while the initiated found the science itself proportionally less advanced. Nothing could be more wildly arbitrary than the names then affixed to the various chemical bodies, forming a jargon in which men and gods, beasts, fish, and fowl, and things of the inanimate creation, all found a namesake which the inven-

tor intended according to his varying whim; now as a compliment to heaven, and now as a mark of regard for aught that struck his fancy in or upon the earth. Nay it would seem that some men of very perverse inclination endeavoured by *the name* to mislead and deceive the uninitiated as to *the thing*;—as it is difficult in any other way to account for a fact such as that three most deadly poisons, the acetate of lead, the chloride of antimony, and the chloride of arsenic respectively, should have been styled the *sugar* of lead, and the *butter* of antimony and of arsenic. In fine, system was unknown,—there was no co-operation, but each in his turn, in this important work, invented for himself; and the greater part of the names thus bestowed have no reference to the subject designated, and are totally independent of methodical arrangement.

That after the total revolution which the science had undergone, it could continue much longer to be tolerated, was impossible; and so early as 1782, Guyton, the last of the great French chemists who acceded to the new doctrines, was nevertheless the first to furnish a memoir to the Academy proposing a new chemical nomenclature. So soon, therefore, as he became a convert to the new theory, the four leading chemists in France set about providing for the exigencies of the science, by furnishing it with a new methodical nomenclature.

The first principle in planning the new nomenclature was to connect the words with the things they were intended to represent, as is shown in the only words they truly invented, oxygen, hydrogen, and azote;—the next was so to methodize them, as to present a connected view of the chemical facts then known, at the same time endeavouring to provide for the future extension of the science. The roots of new denominations employed to express bodies of recent discovery were drawn from the Greek language, partly to avoid entirely any connexion with the barbarous system previously used, and partly because this mode afforded a facility of expressing a compound substance by an easy compound name, at the same time that, by varying the termination, it was easy to mark the different states of the substance so compounded. Thus these terminations are the same in analogous substances, and to name them conveys at once the nature of the composition to which each is appropriated; and by this method there was introduced the greatest precision and accuracy into the whole science, in which system immediately took the place of chaos.

Of the great benefits conferred by this new nomenclature on chemistry, it is impossible to doubt; and of the philosophical views on which it was constructed and arranged, the success with which for many years it adapted itself perfectly to every improvement in the science, is sufficient evidence. Indeed, it

is only within these few years that the new views which have been taken of the nature of chlorine and fluorine, the discovery of iodine and cyanogen, the decomposition of the alkalies, and the electro-chemical theory, having together introduced more enlarged and philosophical ideas of the nature of combustion and of chemical affinity, than were entertained by Lavoisier, Berthollet, and their associates, a corresponding modification of their nomenclature is become necessary. The recent doctrine of chemical equivalents too renders this reform still more requisite, and promises to give a degree of mechanical precision to chemical nomenclature, such as the French chemists could not possibly have imagined or anticipated. The difficulty now is, to bring the leading chemists of Europe to concur in any one method or set of principles in introducing the innovation. Each has his own peculiar ideas on the subject, and for want of some centre of reunion, some mode of having a full discussion of their separate opinions, there is as yet no immediate prospect of even a provisional nomenclature, however much its want may be felt to be injurious to the interests of science.

We now approach a brilliant period in the life of Berthollet, who had not yet however completed his 40th year. In 1787, by his essay on the Composition and Properties of Prussic Acid, he gave a striking proof of the independence of a mind which ever judged freely for itself, and thereby often rose superior to the prejudices of the day. It was, as has been previously noticed, one of the doctrines of the theory of Lavoisier, that oxygen is the acidifying principle, and that no acid exists without its presence. So soon as the leading features of this theory began to be received by chemists as correct, an implicit assent to all its details was given by almost every chemist, save Berthollet. We have already seen that in his memoir on Sulphuretted Hydrogen Gas, in 1778, he stated it to perform all the functions of an acid, and now again, in this Essay on the Nature of Prussic Acid, he found himself enabled, after the successful issue of an analysis, attended by no ordinary difficulties, to declare, that *prussic acid contains no oxygen*. He showed that it nevertheless performs every function of an acid, having affinity for and combining with alkalies, neutralizing them, and forming with them crystallizable compounds, and being again displaced from these combinations by the more powerful acids. The analogy to an unbiassed mind was complete; yet Berthollet's opinion, that acids may exist without the presence of oxygen, gained not a single convert. The new theory now found an implicit acquiescence in its errors, not less unreasonable than the reluctant and tardy assent which had been yielded to its truths. Nay, so undisputed became its authority, even in those points in which each man's own experience should have been his guide, that

when Berthollet, nine years after this, again resumed the subject, again investigated the nature of sulphuretted hydrogen, and again confirmed every former statement he had made, though he had long been confessedly one of the first French chemists, again found the same ill success in attempting to establish an important truth which has only commanded general assent since the recent era to which we have already alluded.

But the year 1787 is further remarkable as the date of the publication of some of Berthollet's most important researches into the nature of chlorine. He had already given to the world his first memoir on this subject in 1785: it was one which came repeatedly under his notice, and on each occasion his investigations were attended by results the most important; at one time to the interests of science, at another to the advancement of the arts. His experiments on this substance may be divided into three branches. The first regards the nature of simple chlorine; the second, its combination with oxygen; and the third, its property of destroying vegetable colour.

The history of M. Berthollet's researches into the constitution of chlorine is one of the greatest interest and instruction. The views which he adopted have been proved by subsequent experiments to be erroneous; but the process of reasoning by which he arrived at his results appeared so plain, his conclusions seemed so inevitable, and all the phenomena were by its means so satisfactorily accounted for, that during a period of twenty-five years, his theory was universally received. Its overturn has been the consequence only of the discovery of facts unknown at the time of its formation, the metallic basis of the alkalies, the new substance iodine, and several others, all of which are closely analogous in their properties with chlorine.

Scheele, who discovered chlorine in 1774, had also the great merit of taking a correct view of its constitution. He called it *dephlogisticated muriatic acid*, or, in modern terms, *muriatic acid deprived of its hydrogen*. Berthollet, on the contrary, considered *muriatic acid* to be the simple (or at least the till then undecomposed) body, and he regarded chlorine as a compound of this simple substance and oxygen. And his reasoning on the subject seemed then to be close and irrefragable.

If muriatic acid be digested over the black oxide of manganese, a portion of it is decomposed, and separates in the state of chlorine gas; the remaining portion is found to hold in solution the oxide of manganese at an inferior degree of oxidation. Of course, the black oxide has also undergone decomposition, and given up a portion of its oxygen; but not a trace of this gas remains in the liquid. From this, Berthollet concluded, that it had gone off with the chlorine, and formed part of that substance:

in place of which, the modern account is that, muriatic acid being compounded of chlorine and hydrogen, the hydrogen combines with the excess of oxygen in the black oxide of manganese, forming *water*, while the chlorine, a simple substance, is set at liberty. This experiment, however, seemed to Berthollet and to all his brother chemists, to furnish a convincing synthetical demonstration of the composition of chlorine. His analytical proof was the following:

An aqueous solution of chlorine, exposed to the light for some days, gave off a quantity of oxygen gas amounting to nearly one-third of its volume. After this evolution had ceased, no trace of chlorine appeared behind, the only substance remaining in the liquid being muriatic acid. Here, then, the chlorine seemed to be decomposed into muriatic acid and oxygen. Berthollet measured the volume of oxygen gas evolved, and estimating the quantity of muriatic acid formed by throwing it down with nitrate of silver, he found himself able to calculate the proportions of the supposed constituents of chlorine. The modern account of the phenomena just mentioned is, that a certain quantity of *water* undergoes decomposition, its hydrogen combining with the chlorine, and forming muriatic acid, while its oxygen escapes in the state of gas. Berthollet, however, having his views of the nature of chlorine now rested on apparently the strongest of all grounds, changed the appellation of Scheele into that of *oxygenized muriatic acid*; a name which it retained until Sir H. Davy published his new view of its constitution in 1810.

On this occasion, it is proper to observe, that the very extent of Berthollet's acquaintance with chemical facts tended to mislead his views when once they had taken a wrong bias, and to strengthen the confidence he felt in this erroneous opinion, Lavoisier had shortly before this shown that no metal can unite with an acid, unless it be in the first place combined with a dose of oxygen. Now, if metallic zinc be put into an aqueous solution of chlorine, it dissolves there as silently as sugar does in water. There is no effervescence, no evolution of gas, as is ordinarily the case during the solution of metals in acids, and from the liquid, by the proper chemical reagents, there may be separated muriatic acid and oxide of zinc. Here, said Berthollet, *the oxygenized muriatic acid imparts its oxygen to the zinc*, and then, *the disengaged muriatic acid combines with the newly-formed oxide, and produces muriate of zinc*. In place of which the modern explanation is, that the chlorine acts directly upon the zinc; and that when these two substances are obtained in the state of muriatic acid and oxide of zinc, a corresponding quantity of water has undergone decomposition; its hydrogen and oxygen having united respectively with the chlorine and the metal.

Another of the leading corroborations of this theory of the nature of chlorine, it is surely interesting to give, in order to explain fully the grounds on which the whole chemical world, with Berthollet at their head, went into a great error at a period when investigation was peculiarly alive, and continued in it during the active researches of a quarter of a century. It was this: the weaker acids are unable of themselves to expel the excess of oxygen from the black oxide of manganese, so as to unite with the salifiable oxide; but when aided by any substance, sugar for example, having a strong affinity for oxygen, the salifiable oxide is then developed, which the acid immediately dissolves. This Berthollet held to be the precise account of the phenomena attending the solution of the black oxide of manganese in muriatic acid. The acid has a strong affinity for oxygen; it has also a strong affinity for the salifiable oxide of manganese; hence, a portion of it combines with the excess of oxygen, and flies off in the state of oxygenized muriatic acid: the remainder combines simultaneously with the salifiable oxide thus developed, and forms along with it the common muriate of manganese.

There are few more interesting explanations of chemical phenomena than those on the one hand urged with so much force by Berthollet in support of his theory, and those on the other which modern science is now enabled to offer in complete subversion of it. It is entertaining to consider each of these views even separately, and it is highly useful to compare them with each other. We thus find as the result of all the intellect and research which has been brought to bear on the question, that Scheele has the praise of having truly viewed the nature of that important substance, which he had also the merit of discovering; while to Berthollet belongs the scarcely smaller honour of having overturned the doctrine of Scheele, and of having so firmly erected his own hypothesis in its stead, that it remained unshaken and almost unquestioned, until our illustrious countryman Davy succeeded in restoring chlorine once more to its original character.

(To be continued.)

## ARTICLE II.

*On the Chloride of Titanium.* By Mr. E. S. George.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

Grove Terrace, Leeds, Nov. 13, 1824.

IN a paper published in the *Philosophical Transactions* for 1823, Dr. Wollaston states, that the substance from Merthyr

Tydvil, which he has shown to be metallic titanium, occurs also at the Low Moor Iron Works, near Bradford, Yorkshire. Having a short time ago an opportunity to examine the foundation of a blown out furnace at the Low Moor Iron Works, I found the upper part of the stone, upon which the melted metal rests, completely penetrated by metallic iron, sulphuret of iron, and carbonaceous matter, amongst which brilliant cubes of metallic titanium were thickly dispersed.

Upon a portion of this substance reduced to a coarse powder I poured muriatic acid; a large quantity of hydrogen and sulphuretted hydrogen gases was extricated, and after ebullition in excess of acid, the iron and earths contained in the slag were dissolved, leaving brilliant cubes of titanium having a colour between that of copper and gold and possessing great metallic brilliancy, mixed with grains of silex; the carbonaceous part had floated away with the muriatic solutions.

Having removed the grains of silex, 60 grains of the metallic titanium were placed in a glass tube, and a current of chlorine (from which all moisture had been removed by dry chloride of calcium), passed over them, no action was perceptible, nor was the lustre in the least impaired; on heating to ignition the part of the tube in which the titanium was placed, a fluid gradually condensed in the cool part of the tube, and was collected by gently inclining it.

This fluid is transparent and colourless; it possesses considerable density; on exposure to the atmosphere, it emits dense white fumes, having a pungent odour resembling, but not nearly so offensive as, chlorine; the dense fumes appear to depend upon the presence of moisture; it boils violently at a temperature a little higher than  $212^{\circ}$  Fahr. and is recondensed without decomposition: on the addition of a drop of water to a few drops of this liquid, a very rapid, almost explosive disengagement of chlorine ensued, attended by a considerable elevation of temperature, and when the water is not in excess, a solid salt is formed.

This salt is very soluble in water, deliquescent, and its solution possesses all the properties of muriate of titanium, giving a brownish red precipitate with prussiate of potash, a dark red with infusion of galls; with pure potash a gelatinous precipitate, soluble in excess of muriatic acid, and after subsidence nitrate of silver, occasioning in the supernatant fluid a precipitate of chloride of silver: ammonia throws down a white precipitate from the solution.

A salt possessing the same properties crystallizes in the interior of the tube when the chlorine is not freed from hygrometric moisture.

To ascertain the composition of the two substances, upon 14.6

grains of the fluid in a long test tube, I dropped a weighed portion of water very gradually; chlorine was disengaged rapidly, and the temperature of the tube became considerably elevated; after cooling I found the loss of weight 4 grs.: the solution gave with gallic acid a dark red precipitate. This fluid is the perchloride of titanium, since, by the separation of chlorine, it is converted into the protochloride, which becomes the muriate by solution.

From the difficulty attendant upon the drying of the salt (whether formed by crystallization in the tube, or by the decomposition of the perchloride), without rendering a part insoluble; I added water to a solution of muriate of titanium formed by the decomposition of the perchloride by water, and divided the solution into two equal parts; from the one I precipitated the oxide of titanium by potash; the precipitate when dried weighed 7 grains, and from the other I precipitated the chlorine by nitrate of silver; the chloride of silver when dried weighed 15 grains, containing chlorine 3.6. Hence the muriate of titanium is composed of oxide of titanium 7, muriatic acid (chlorine 3.64 + hydrogen 1-) 3.74. Supposing the muriate to be composed of 1 atom muriatic acid and 1 atom oxide of titanium, the oxide is the protoxide resulting from the combination of 1 atom of oxygen with 1 atom titanium, and the weight of titanium will be 61.2, it is probable that the true number is 64, as indicated by the experiments of M. Rose. From this analysis, the composition will be,

Muriate of Titanium.

Oxide of titanium . . . . .	7.00
Muriatic acid . . . . .	3.74

Or as Protochloride.

Titanium . . . . .	6.12
Chlorine . . . . .	3.64

Perchloride Titanium.

Titanium . . . . .	6.66
Chlorine . . . . .	7.91

*I remain, your obedient servant,*

E. S. GEORGE.

## ARTICLE III.

Corrections in Right Ascension of 37 Stars of the Greenwich Catalogue. By James South, FRS.

	γ Pegasi	Polaris	α Arietis	α Ceti	Aldebaran	Capella	Rigel	γ Tauri	α Orionis
Mean AR 1825.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
	5 0 4 14.25	0 58 17.50	1 57 19.77	2 33 8.58	4 25 53.44	5 3 46.61	5 6 8.00	5 15 14.29	5 45 42.18
Jan. 1	+ 0.93"	+ 9.21	+ 1.71"	+ 1.87"	+ 2.41"	+ 3.35"	+ 2.30	+ 2.79	+ 2.48"
2	91	8.52	70	86	41	35	30	79	48
3	90	7.83	68	85	40	35	30	79	49
4	88	7.14	67	85	40	35	30	79	49
5	87	6.45	66	84	40	35	30	79	49
6	86	5.75	64	83	39	35	30	79	50
7	85	5.04	63	82	39	35	30	80	50
8	84	4.34	62	82	39	34	29	80	51
9	83	3.64	61	81	38	34	29	80	51
10	82	2.93	59	80	38	34	29	80	52
11	81	2.26	58	79	37	33	28	79	52
12	80	1.59	56	78	37	33	28	79	52
13	79	0.93	55	77	36	32	28	79	52
14	78	+ 0.6	54	76	35	31	27	78	52
15	76	- 0.43	52	75	35	31	27	78	52
16	75	1.17	51	74	34	30	26	78	51
17	74	1.91	50	73	33	29	26	78	51
18	73	2.65	49	72	33	28	25	77	51
19	73	3.40	47	71	32	27	25	77	51
20	72	4.14	46	69	31	26	24	77	51
21	71	4.81	45	68	30	25	24	76	50
22	70	5.48	43	66	29	24	23	76	50
23	69	6.16	42	65	28	23	22	75	50
24	68	6.83	41	64	27	22	21	75	49
25	68	7.50	39	63	26	20	20	74	49
26	67	8.16	38	61	25	19	20	73	48
27	66	8.81	37	60	24	18	19	72	48
28	65	9.47	36	59	23	17	18	71	47
29	64	10.12	34	58	22	15	17	70	47
30	63	10.78	33	56	21	14	16	69	46
31	62	11.40	31	55	20	12	15	68	45
Feb. 1	61	12.02	30	53	18	11	14	67	45
2	61	12.61	28	52	17	09	13	66	44
3	60	13.25	27	51	16	08	11	65	43
4	59	13.87	25	50	15	06	10	64	42
5	58	14.48	24	49	14	05	09	62	41
6	56	15.09	22	47	12	03	08	61	40
7	56	15.71	21	46	11	02	07	60	39
8	55	16.32	19	45	10	00	06	59	38
9	55	16.93	18	43	08	2.98	04	57	37
10	55	17.48	16	42	07	96	02	56	36
11	54	18.03	15	40	05	94	01	54	35
12	53	18.58	14	39	04	92	1.99	53	34
13	53	19.12	13	37	02	90	98	51	32
14	52	19.67	11	36	01	88	96	50	31
15	52	20.20	10	34	00	86	95	49	30
16	51	20.71	09	33	1.99	84	93	48	29
17	51	21.27	07	31	97	82	92	46	28
18	50	21.81	06	30	96	80	90	45	27
19	50	22.34	05	28	94	78	88	43	25
20	50	22.81	03	27	92	76	86	41	24
21	50	23.27	02	25	91	74	85	39	22
22	49	23.74	01	24	89	72	83	38	21
23	49	24.21	0.99	22	87	69	81	36	19
24	49	24.67	98	21	86	67	80	34	18
25	49	25.09	97	19	84	65	78	32	16
26	48	25.50	96	18	82	63	76	30	15
27	48	25.91	95	16	80	61	74	28	13
28	48	26.32	94	15	79	58	73	27	12

	Sirius	Castor	Procyon	Pollux	$\alpha$ Hydræ	Regulus	$\beta$ Leonis	$\gamma$ Virginis	Spica Virg.
Mean AR 1925.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
	6 37 26.11	7 23 25.30	7 30 8.47	7 34 35.85	9 18 59.40	9 59 2.78	11 40 7.50	11 41 34.98	13 15 59.22
Jan. 1	+ 2.38"	+ 2.95"	+ 2.47"	+ 2.84"	+ 2.12"	+ 2.07"	+ 1.56"	+ 1.47"	+ 0.93"
2	39.	97	48	86	14	10	53	56	97
3	39	98	50	87	16	12	56	54	1.00
4	40	3.00	51	89	18	15	60	57	04
5	41	01	53	91	21	18	63	61	07
6	41	03	54	92	23	20	66	64	11
7	42	04	56	94	25	23	69	67	14
8	43	06	58	95	28	25	72	71	18
9	43	07	59	97	30	28	76	74	21
10	44	09	60	98	32	31	79	77	24
11	44	10	61	99	34	34	82	80	28
12	44	11	62	3.00	36	36	85	84	31
13	44	11	63	01	38	38	87	87	35
14	45	12	63	02	40	41	90	90	38
15	45	13	64	03	42	43	93	93	41
16	45	14	65	04	44	45	96	96	45
17	45	15	66	05	46	47	99	99	48
18	46	15	67	05	48	50	2.01	01	51
19	46	16	67	06	49	52	04	2.03	54
20	46	17	68	07	51	54	07	05	57
21	46	17	68	07	52	56	10	08	60
22	46	18	69	08	54	57	13	10	64
23	45	18	69	08	55	59	16	13	67
24	45	19	69	09	56	61	18	15	70
25	44	20	70	09	57	63	21	18	73
26	44	20	70	10	59	64	23	20	76
27	44	20	71	10	60	66	26	23	79
28	44	20	71	11	62	68	28	25	82
29	43	21	72	11	63	69	31	28	85
30	43	21	72	12	64	71	33	30	88
31	43	21	72	12	65	72	35	33	91
Feb. 1	42	21	72	12	66	74	38	35	94
2	42	21	71	12	67	75	40	37	97
3	41	20	71	12	68	76	43	39	2.00
4	40	20	71	12	69	78	45	41	03
5	39	20	71	12	70	79	47	43	06
6	38	20	71	12	71	80	49	45	09
7	37	19	70	12	72	81	52	47	12
8	36	19	70	12	73	83	54	49	15
9	35	19	70	12	73	84	56	51	17
10	34	18	70	11	73	85	58	53	20
11	33	18	69	11	74	86	60	55	22
12	32	17	69	10	74	87	62	57	25
13	31	16	68	10	74	88	64	59	27
14	30	15	68	09	75	89	66	61	30
15	29	15	67	09	75	90	68	63	32
16	28	14	67	08	76	91	70	65	35
17	27	13	66	08	76	92	72	67	37
18	26	13	65	07	76	93	74	69	40
19	24	12	64	06	77	93	75	70	42
20	23	11	63	05	77	94	76	72	45
21	21	10	62	04	77	91	78	73	47
22	20	09	61	03	77	94	79	75	49
23	18	08	60	02	77	94	81	76	52
24	17	07	59	01	77	95	82	77	54
25	15	05	58	00	76	95	83	78	56
26	13	04	57	2.99	76	95	84	79	59
27	12	03	56	97	76	95	85	81	61
28	10	02	55	96	76	95	86	83	63

	Arcturus	2 $\alpha$ * Libra	$\alpha$ Cor. Bor.	$\alpha$ Serpent.	Antares	$\alpha$ Herculis	$\alpha$ Ophiuchi	$\alpha$ Lyrae	$\gamma$ Aquilae
Mean Alt. 1825.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
	14 7 41.06	14 41 12.92	15 27 16.97	15 35 39.42	16 18 41.57	17 6 40.44	17 26 49.02	18 31 1.01	19 37 56.55
Jan. 1	+ 0.54"	+ 0.53"	- 0.06"	+ 5.16"	+ 0.14"	- 0.25"	- 0.26"	- 0.94"	- 0.25"
2	57	57	03	19	17	23	24	93	24
3	60	60	00	22	20	20	22	92	24
4	63	64	+ 0.03	25	23	18	20	91	23
5	66	67	06	28	26	16	18	90	23
6	69	70	10	30	29	14	16	89	22
7	72	73	13	33	32	11	14	88	22
8	75	77	16	36	35	09	12	87	21
9	78	80	19	39	38	07	10	86	20
10	81	83	22	42	41	05	08	84	19
11	84	86	25	45	44	03	06	83	18
12	87	89	28	48	47	00	04	81	17
13	91	93	31	51	50	+ 0.02	01	80	16
14	94	96	34	54	53	04	+ 0.01	78	15
15	97	99	37	57	56	07	03	77	14
16	1.00	1.02	40	60	59	09	05	75	13
17	03	06	43	63	62	12	07	74	12
18	06	09	46	66	65	14	09	72	11
19	09	12	49	69	69	17	12	70	10
20	13	15	52	72	72	19	14	68	08
21	16	19	55	75	75	22	16	66	07
22	19	22	58	78	79	24	19	64	05
23	22	25	61	81	82	27	21	62	04
24	26	29	64	84	85	29	23	60	02
25	29	32	68	87	88	32	26	58	01
26	32	36	71	90	92	34	28	56	+ 0.01
27	35	39	74	93	95	37	31	54	02
28	38	42	77	96	98	39	33	52	03
29	41	46	80	99	1.02	42	36	50	05
30	44	49	84	1.02	05	45	38	48	06
31	47	52	87	05	08	48	41	46	08
Feb. 1	50	55	91	08	12	51	44	45	10
2	53	59	94	11	15	53	46	44	11
3	56	62	97	14	18	55	49	38	13
4	59	65	1.01	17	22	59	52	36	15
5	62	69	04	20	25	61	55	34	17
6	66	72	07	23	29	64	57	31	19
7	69	75	10	26	32	67	60	28	20
8	72	78	13	29	35	70	63	26	22
9	75	81	17	32	39	73	66	23	24
10	78	84	20	35	42	76	69	20	26
11	81	87	23	38	46	79	71	18	28
12	84	90	26	41	49	82	74	15	30
13	86	93	29	44	53	85	77	12	31
14	89	96	32	47	56	88	80	09	33
15	92	99	36	50	60	91	82	07	35
16	95	2.03	39	53	63	94	85	04	37
17	98	06	42	56	66	96	88	02	39
18	2.01	09	45	59	70	99	91	+ 0.01	41
19	03	12	48	62	73	1.02	93	04	43
20	06	15	51	65	76	05	96	07	45
21	08	18	54	68	80	08	99	10	48
22	11	21	57	71	83	10	1.02	13	50
23	13	24	60	74	86	13	05	16	52
24	16	26	63	77	90	16	07	19	54
25	18	29	66	80	93	19	10	22	56
26	21	32	69	83	97	21	13	25	59
27	23	35	72	86	2.00	24	16	28	61
28	26	37	75	89	04	27	19	31	63

\* Mean Alt. of 1 $\alpha$  Libra 14h 41' 1.50"

	$\alpha$ Aquilæ	$\beta$ Aquilæ	$\gamma$ Capri.	$\alpha$ Cygni	$\alpha$ Aquarii	Fomalhaut	$\alpha$ Pegasi	$\alpha$ Androm.
Mean AR 1825.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
	19 42 14.84	19 46 43.20	20 8 20.34	20 35 28.24	21 56 47.77	23 47 57.67	22 56 3.16	23 59 21.74
Jan. 1	- 0.22"	- 0.19"	- 0.03"	- 0.87"	- 0.27"	+ 0.50"	+ 0.50"	+ 0.87"
2	21	18	03	87	27	49	49	85
3	21	18	02	88	26	48	48	84
4	20	17	02	88	26	47	47	82
5	20	17	01	89	26	46	46	81
6	19	16	01	89	25	45	46	79
7	19	16	00	89	25	44	45	78
8	18	15	00	90	24	43	44	76
9	18	15	+ 0.01	90	24	41	43	75
10	17	14	01	91	23	40	42	74
11	16	13	02	91	23	39	41	73
12	15	12	03	91	23	38	41	72
13	14	11	04	91	23	38	40	70
14	13	10	04	91	23	37	39	69
15	12	09	05	90	23	36	38	68
16	11	08	06	90	23	35	38	67
17	10	07	07	90	22	34	37	65
18	09	06	08	89	22	34	37	64
19	08	05	09	89	22	33	36	62
20	06	03	10	89	22	32	35	62
21	05	02	11	89	22	31	34	61
22	03	00	12	88	22	31	34	60
23	02	+ 0.01	14	88	22	30	33	59
24	01	03	15	87	22	30	33	58
25	+ 0.01	04	16	87	22	29	32	57
26	02	05	17	87	22	29	32	56
27	04	06	18	86	23	28	31	55
28	05	07	19	86	23	28	31	54
29	07	09	20	85	23	27	30	53
30	08	10	22	85	23	27	30	52
31	10	12	24	84	23	27	30	51
Feb. 1	11	13	25	83	23	27	30	50
2	13	15	27	82	21	27	29	50
3	15	17	28	81	24	26	29	49
4	16	19	30	79	25	26	29	48
5	18	20	31	78	25	26	29	47
6	20	22	33	77	25	26	29	46
7	22	24	34	76	25	25	28	46
8	23	25	36	75	26	25	28	45
9	25	27	38	73	26	25	28	44
10	27	29	40	72	27	25	28	43
11	29	31	42	70	28	25	28	43
12	31	32	44	69	28	26	28	42
13	33	34	45	67	29	26	28	42
14	35	36	47	66	30	26	28	41
15	37	38	49	64	31	26	29	41
16	39	40	51	63	31	26	29	40
17	41	42	53	61	32	26	29	40
18	43	44	55	60	33	27	29	39
19	45	46	57	58	34	27	29	38
20	47	48	59	56	35	28	29	38
21	49	50	61	54	36	28	30	37
22	52	52	63	52	37	29	30	37
23	54	54	65	50	38	29	30	37
24	56	57	67	48	39	30	30	36
25	58	59	69	46	40	30	30	36
26	60	61	71	44	41	31	31	35
27	62	63	74	43	42	31	31	35
28	65	66	76	41	43	32	32	34

\* Mean AR of  $\gamma$  Capricor. 20<sup>h</sup> 7<sup>m</sup> 56.55".

Mean AR 1825.	$\gamma$ Pegasi	Polaris	$\alpha$ Arietis	$\alpha$ Ceti	Aldebaran	Capella	Rigel	$\delta$ Tauri	$\alpha$ Orionis
	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
0	4 14 25	0 58 17.50	1 57 19.77	2 53 8.56	4 25 33.44	5 3 46.61	5 6 8.00	5 15 14.29	5 45 42.18

March	1	+ 0.47"	-26.73"	+ 0.93"	+ 1.13"	+ 1.77"	+ 2.56"	+ 1.71"	+ 2.25"	+ 2.10"
2	47	27.08	92	12	75	54	69	23	08	
3	47	27.44	91	10	74	52	67	21	07	
4	47	27.80	90	09	72	50	66	20	05	
5	47	28.15	89	08	71	47	64	18	03	
6	47	28.50	88	06	69	45	62	16	02	
7	47	28.82	87	05	67	43	60	14	00	
8	47	29.14	86	04	66	41	58	12	1.98	
9	47	29.46	85	03	64	39	57	11	97	
10	47	29.78	84	01	63	36	55	09	95	
11	47	30.10	83	00	61	34	53	07	93	
12	47	30.35	82	0.99	60	32	51	05	91	
13	48	30.59	81	98	58	29	50	03	90	
14	48	30.83	81	97	56	27	48	02	88	
15	48	31.07	80	96	55	24	46	00	86	
16	49	31.31	79	95	53	22	44	1.98	85	
17	49	31.49	79	94	52	20	43	96	83	
18	50	31.68	78	93	50	17	41	94	81	
19	50	31.86	77	92	49	15	39	92	79	
20	51	32.04	77	91	47	13	38	91	78	
21	51	32.22	76	90	45	10	36	89	76	
22	52	32.35	76	89	44	08	34	87	74	
23	53	32.47	75	88	42	06	32	85	73	
24	53	32.59	75	87	41	04	31	84	71	
25	54	32.71	74	86	39	02	29	82	70	
26	55	32.83	74	85	38	00	27	80	68	
27	55	32.87	73	84	36	1.98	25	78	67	
28	56	32.91	73	83	35	96	23	76	65	
29	57	32.95	73	82	34	94	22	75	64	
30	58	32.98	72	81	32	91	20	73	62	
31	59	33.01	72	81	31	89	18	71	60	

Mean AR 1825.	Sirius	Castor	Procyon	Polaris	$\alpha$ Hydre	Regulus	$\beta$ Leonis	$\delta$ Virginis	Spica Virg.
	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
0	6 37 26.11	7 23 25.30	7 30 8.47	7 34 35.85	9 18 59.49	10 59 2.78	11 40 7.80	11 41 34.98	13 15 59.22

March	1	+ 2.08	+ 3.00	+ 2.51	+ 2.95	+ 2.76	+ 2.96	+ 2.87	+ 2.84	+ 2.65
2	06	2.99	53	94	75	96	88	85	67	
3	05	97	52	92	75	96	89	86	69	
4	03	96	51	91	75	96	90	87	71	
5	01	95	49	90	74	96	91	88	73	
6	1.99	93	48	89	74	96	92	89	75	
7	97	92	47	87	73	96	93	90	77	
8	96	90	46	86	73	96	94	91	79	
9	94	88	45	85	72	96	95	92	81	
10	93	87	44	84	71	96	96	93	83	
11	91	86	42	82	70	96	96	94	84	
12	90	84	41	80	69	95	97	95	86	
13	88	83	40	79	68	95	98	95	87	
14	86	81	38	77	68	95	98	96	89	
15	84	79	37	76	67	94	99	96	91	
16	82	77	36	74	66	94	99	97	92	
17	80	76	35	73	66	93	3.00	98	94	
18	79	74	33	71	65	93	00	98	95	
19	77	72	32	68	64	92	01	98	97	
20	75	70	31	67	63	92	01	99	98	
21	73	69	29	66	62	91	02	99	99	
22	71	67	27	64	61	91	02	99	3.00	
23	69	65	26	62	60	90	02	99	01	
24	67	64	24	61	59	89	02	3.00	02	
25	65	62	23	59	58	88	02	00	03	
26	63	60	21	57	57	88	02	00	04	
27	61	58	19	55	56	87	02	00	06	
28	59	56	18	53	55	86	02	00	07	
29	57	54	16	52	54	85	02	01	08	
30	55	52	14	50	53	85	02	01	09	
31	53	50	12	48	51	84	02	01	10	

Mean AR 1825.	Arcturus h. m. s. 14 7 41.06	$\alpha$ Libræ h. m. s. 14 41 12.92	$\alpha$ Cor. Bor. h. m. s. 15 27 16.97	$\alpha$ Serpent. h. m. s. 15 55 39.42	Antares h. m. s. 16 18 41.57	$\alpha$ Herculis h. m. s. 17 6 49.44	$\alpha$ Ophiuchi h. m. s. 17 26 49.02	$\alpha$ Lyræ h. m. s. 18 31 1	$\gamma$ Aquilæ h. m. s. 19 37 56.55
March 1	+ 2-28"	+ 2-40"	+ 1-77"	+ 1-91"	+ 2-07"	+ 1-30"	+ 1-22"	+ 0-34"	+ 0-66"
2	30	43	80	94	10	33	25	37	68
3	33	46	83	97	14	36	28	40	71
4	35	48	86	99	17	39	31	44	73
5	38	51	89	2-02	20	42	34	47	76
6	40	53	92	05	23	45	36	50	78
7	42	56	95	08	26	48	39	53	80
8	44	58	98	10	30	51	42	57	83
9	47	61	2-01	13	33	54	45	60	85
10	49	64	04	16	36	57	48	63	88
11	51	66	06	18	40	60	51	66	90
12	53	69	09	21	43	63	54	69	93
13	55	71	12	24	46	66	57	73	95
14	57	73	14	26	50	68	60	76	98
15	59	75	17	29	53	71	63	79	1-00
16	61	78	20	31	56	74	66	83	03
17	63	80	22	34	59	77	69	86	05
18	65	83	24	36	62	80	72	89	08
19	67	85	27	39	65	82	75	92	10
20	69	87	29	41	68	85	78	96	13
21	70	89	31	44	71	88	80	99	16
22	72	91	33	46	74	91	83	1-02	19
23	73	93	35	48	77	93	86	06	21
24	74	95	38	51	80	96	88	09	24
25	76	97	40	53	83	99	91	12	26
26	77	99	42	55	86	2-01	94	16	29
27	79	3-01	45	57	89	03	97	19	31
28	80	03	47	59	92	07	2-00	22	34
29	82	05	49	61	95	09	02	25	36
30	83	07	51	63	98	12	05	29	39
31	85	09	53	65	3-01	15	08	32	42

Mean AR 1825.	$\alpha$ Aquilæ h. m. s. 19 42 14.84	$\beta$ Aquilæ h. m. s. 19 46 43.22	$2\alpha$ Capricor. h. m. s. 20 8 26.34	$\alpha$ Cygni h. m. s. 20 35 28.24	$\alpha$ Aquaræ h. m. s. 21 56 47.77	Pomahaan h. m. s. 22 47 57.67	$\alpha$ Pegasi h. m. s. 22 56 3.16	$\alpha$ Androm. h. m. s. 23 59 21.74
March 1	+ 0-67"	+ 0-65"	+ 0-78"	- 0-39"	+ 0-41"	+ 0-32"	+ 0-32"	+ 0-34"
2	69	70	80	37	45	33	32	34
3	72	73	83	34	47	34	33	34
4	74	75	85	32	48	34	34	34
5	76	78	87	30	49	35	35	34
6	79	80	89	27	50	36	35	34
7	81	83	91	25	51	37	36	35
8	83	85	94	22	52	38	37	35
9	86	88	97	20	54	39	38	35
10	88	90	99	17	55	40	39	35
11	91	92	1-01	15	57	41	40	35
12	93	95	01	12	59	42	41	35
13	95	97	06	09	60	43	42	36
14	98	1-00	09	07	62	45	42	36
15	1-01	02	11	04	64	46	43	37
16	03	05	14	01	66	47	44	37
17	06	07	16	+ 0-02	67	48	45	38
18	08	10	19	03	69	50	46	38
19	11	12	21	07	71	51	47	39
20	14	15	23	10	73	52	48	39
21	17	17	26	13	74	54	51	40
22	20	20	29	16	76	56	52	40
23	22	23	32	19	78	57	54	41
24	25	25	34	22	80	59	55	42
25	27	28	37	26	82	61	57	43
26	30	30	40	29	83	63	58	44
27	33	33	43	32	85	64	60	45
28	35	35	46	35	87	66	61	46
29	38	38	48	38	89	68	63	47
30	40	40	51	42	91	69	64	48
31	43	43	54	45	93	71	66	49

## ARTICLE IV.

*On the Structure of Pearls, and on the Chinese Mode of producing them of a large Size and regular Form.* By John Edward Gray, MGS.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

Dec. 10, 1824.

PEARLS are merely the internal pearly coat of the shell, which has assumed, from some extraneous cause, a spherical form; they are, like the shell, composed of concentric coats formed of perpendicular fibres; consequently when broken they exhibit concentric rings and fibres radiating from a central nucleus usually consisting of a grain of sand or some other body which has irritated the animal. A pearl having been once formed, the animal continues to increase its size by the addition of fresh coats, perhaps more rapidly deposited on it than on the rest of the shell, as the prominence remains a source of irritation.

The pearls are usually of the colour of the part of the shell to which they are attached. I have observed them white, rose coloured, purple,\* and black, and they are said to be sometimes of a green colour; they have also been found of two colours, that is, white with a dark nucleus, which is occasioned by their being first formed on the dark margin of the shell before it is covered with the white and pearly coat of the disk, which, when it becomes extended over them and the margin, gives them that appearance.

Pearls vary greatly in their transparency. The pink are the most transparent, and in this particular they agree with the internal coat of the shell from which they are formed, for these pearls are only formed on the pinnæ, which internally are pink and semitransparent, and the black and purple specimens are generally more or less opaque.

Their lustre, which is derived from the reflection of the light from their peculiar surface produced by the curious disposition of their fibres, and from their semitransparency and form, greatly depends on the uniformity of their texture and colour of the concentric coats of which they are formed. That their lustre does depend on their radiating fibres may be distinctly proved by the inequality of the lustre of the "Columbian pearls" which are filed out of the thick part near the hinge of the pearl

\* I can with certainty inform the anonymous author in the *Edinburgh Philosophical Journal*, No. xxi. p. 44, who observes, that "in the British Museum there *is* or *was* a famous pink pearl," that there not only now *is* one, but three of these pearls, as he might have convinced himself, for they have been exposed to the public now for these last three or four years to my own knowledge.

oyster, *Avicula Margaritifera*,\* so that they are formed like that shell of transverse laminæ, and they consequently exhibit a plate of lustre on one side which is usually flat, and are surrounded by brilliant concentric zones, which show the places of the other plates, instead of the even beautiful soft lustre of the true pearls.

Some time ago in examining the shells in the British Museum, I observed a specimen of *Barbala plicata*,† with several very fine regular shaped semiorbicular pearls of most beautiful water, and on turning to their superb collection of pearls, I found several fragments of the same shell with similar pearls, and on the attentive examination of one of them, which was cracked across, I observed it to be formed of a thick coat consisting of several concentric plates formed over a piece of mother-of-pearl roughly filed into a plano-convex form, like the top of a mother-of-pearl button. On examining the other pearls they all appeared to be formed on the same plan. In one or two places where the pearl had been destroyed or cut out, there was left in the inside of the shell a circular cavity with a flat base, about the depth, or rather less, than the thickness of the coat that covered the pearls, which distinctly proves that these pieces of mother-of-pearl must have been introduced when the shells were younger and thinner; and the only manner that they could have been placed in this part of the shell must be by the introduction of them between the leaf of the mantle and the internal coat of the shell; for they could not have been put in through a hole in the shell, as there was not the slightest appearance of any injury near the situation of the pearls on the outer coat.

Since these observations I have tried the experiment of introducing some similar pieces of mother-of-pearl (which may now be truly so called) into the shell of the *Anodonta Cygneus* and *Unio Pictorum*, which I have again returned to their natural habitation; and I am in hopes that some persons who have more convenience, and are better situated for the purpose, will repeat these experiments, especially with the *Unio Margaritifera*. I found the introduction of the basis of the pearl attended with very little difficulty, and I should think very little absolute pain to the animal; for it is only necessary that the valves of the shell should be forced open to a moderate breadth, and so kept for a few seconds by means of a stop, and that then the basis should be introduced between the mantle and the shell, by

\* I have placed this shell with the *Avicula*. as, when young, it has the teeth of that genus; and I have seen an old specimen which would scarcely agree with Lamarck's "*Cardo edentulus*,"

† This shell was described and figured by Dr. Leach in his Zoological Miscellany under the name of *Dipsas plicatus*, but *Dipsas* has been used as a genus of Annulosa. I have, therefore, adopted Mr. Humphrey's name; Dr. Leach had changed it to *Ap-pius plicatus*.—It may be the *Mytilus plicatus* of Solander's MSS. confounded by Dillwyn with the *Mytilus dubius* of Gmelin, but the pearls are certainly not "furnished with stalks," as they are described in the Portland Catalogue, p. 59, to be in that shell.

slightly turning down the former part, and pushing the pieces to some little distance by means of a stick, when the stop may be withdrawn, and the animal will push the basis into a convenient place by means of its foot, and of the 30 or 40 bases which I thus introduced, only one or two were pushed out again, and these I do not think had been introduced sufficiently far. In several which I afterwards destroyed, I found that the bases were always placed near the posterior slope of the shell, where the pearls are situated in the *Barbula*.

If this plan succeed, which I have scarcely any doubt it will, we shall be able to produce any quantity of as fine pearls as can be procured from abroad. My reason for believing that this manner of forcing the animals of the freshwater bivalves to produce pearls, is the invention of the Chinese, a nation celebrated for their deceptions and trick, is that in looking over the collection of shells of Mr. G. Humphreys, I observed that a shell of this species (the second perfect one that I have seen) was marked as having come from China.

This plan at least is certainly much preferable to the one proposed by Linneus, and by the above quoted anonymous author, as the pearls are all of a regular form, and that the one best suited for setting. In cutting these pearls from the shell, it is necessary that the shell should be cut through, so that the mother-of-pearl button may be kept in its place: for if the back were removed, as it would be were not the shell cut through, the basis would fall out, and then the pearl would be very brittle. The only objection that can be adduced against these pearls is, that their semiorbicular and unequally coloured sides preclude them from being strung, or used any other way than set; but this fault will always be the case with all artificially produced pearls, as the mantle can only cover one side of them; and the only pearls that well answer the purpose of stringing are those found imbedded in the cells in the mantle of the animal.

*Note.*—Since the above was written, my friend Mr. Children has pointed out to me a paragraph in the *Encyclopedia Britannica*, vol. vi. p. 477, in which it is stated, “Pearls are also produced by another artificial process. The shell is opened with great care to avoid injuring the animal, and a small portion of the external surface of the shell is scraped off. In its place is inserted a spherical piece of mother-of-pearl, about the size of a small grain of shot. This serves as a nucleus, on which is deposited the pearly fluid, and in time forms pearl. Experiments of this kind have been made in Finland, and have been repeated in other countries.”

## ARTICLE V.

*On the Use of Animal Charcoal as a Flux.*(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

THE great power of wood charcoal as a flux for minerals and metallic ores has been long known, and extensively taken advantage of in the arts and operations of chemistry, but I am not aware that any application of *animal* charcoal to the same purposes has hitherto been attempted. The following facts, however, it is thought, furnish sufficient grounds for believing that the latter might prove an advantageous substitute for the former, in those cases where its comparative expense would admit of its employment; and they may, therefore, perhaps, obtain a corner in the *Annals of Philosophy* if not occupied with more important matter.

Being in the habit of using animal charcoal as a dentrifice, I nearly filled a brass crucible of moderate size, and about four-tenths of an inch in thickness, with ivory-black, for the purpose of purifying it by re-ignition. The crucible was closed with a cast iron cover, which had a small perforation in it as a vent for the gas which was extricated; and in this state was set in the fire-place of an air furnace, which was commonly employed for heating alkaline lixivia. The fire was not very large, though thoroughly inflamed, and the grate door was left wide open. The crucible soon acquired a red heat (to which it had, prior to this, been frequently exposed), and the gas burned steadily at the aperture in the cover. Being obliged to leave it at this period, on my return in about ten minutes, I was a little surprised to find the iron cover of the crucible lying by itself, and no vestige of the latter apparent in the fire-place. On examining the ash-pit several rugged pieces of brass were found, and two large masses of cinders, firmly compacted together by an upper coating of oxidized brass. In one of these a large stick of the metal was imbedded, which broke with a rough coppery appearance, but on filing immediately displayed its brassy nature.

As the heat by which this was effected appeared to me much inferior to that which brass generally requires for its fusion, I exposed some brass wire, about one-tenth of an inch in diameter, by itself in the same fire, and closed the door. After remaining there nearly half an hour, it was taken out broken into two parts. It was become oxidized, and, as it were, worm-eaten on its surface, and was rendered very brittle in its fracture, but it had not the least appearance of any loss by fusion.

Endeavouring again to effect my purpose with the ivory-black,

I exposed some of it in a *cast iron* crucible, in the same fire-place, and the door open as before. This crucible was only three-tenths of an inch thick, and had occasionally been exposed for short periods to the greatest heat of this fire-place; not expecting, therefore, that it would receive any injury in the present instance, I left it unnoticed for about 20 minutes, by which time the ivory-black had ceased to emit any more gas. It was then taken out, but unfortunately not in the condition in which it was introduced. Nearly half the circumference of the crucible for one and a half inch upwards, and a large part of its bottom, had run into a complete slag upon the opposite side, which happened to have fallen lowest in the fire, and the ivory-black was almost consumed, from the access which the air thus acquired to the inside of the crucible. The cover and upper parts of it had suffered no injury.

From the great heat which brass and particularly cast-iron require for their fusion, and the low degree of it employed in these cases, little doubt can be entertained of the superior agency of animal charcoal as a flux. Both the crucibles it must also be noticed had been formerly used for the very purpose of procuring charcoal from wood in a common grate, when it is conceived the heat was little inferior to that in the present instance, and the chances of their fusion then otherwise equal. It might, therefore, be worth the trouble for those whom it may concern, to make one or two comparative experiments on this subject, with greater accuracy than the preceding, in order to determine it decisively.

F.

Is it not probable that in the experiments above detailed, the metals were converted into phosphurets by the decomposition of the phosphoric acid? and if so, the increased fusibility would probably be derived from this circumstance.—*Edit.*

## ARTICLE VI.

*Astronomical Observations, 1824.*

By Col. Beaufoy, FRS.

*Bushey Heath, near Stanmore.*Latitude  $51^{\circ} 37' 44.3''$  North. Longitude West in time  $1^{\text{h}} 20.93''$ .

Nov. 15.	Emersion of Jupiter's third satellite. ....	{ 12 <sup>h</sup> 20' 52"	Mean Time at Bushey.
		{ 12 22 13	Mean Time at Greenwich.
Nov. 21.	Immersion of Jupiter's first satellite. ....	{ 15 24 22	Mean Time at Bushey.
		{ 15 25 48	Mean Time at Greenwich.
Dec. 7.	Immersion of Jupiter's first satellite. ....	{ 13 58 32	Mean Time at Bushey.
		{ 13 40 13	Mean Time at Greenwich.

Occultation by the Moon.

Oct. 29. Immersion of  $\alpha$  Pisces. ....  $0^{\text{h}} 01' 36.5''$  Siderial Time.

## ARTICLE VII.

## On Paratonnerres.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN.

Dec. 10, 1824.

I HAVE read with much interest the article in your last number by M. Gay-Lussac, on Paratonnerres; and as I have long wished to erect one on my house in London, I should take it as a great favour if you could, in one of your next numbers, devote a few lines to satisfy me and some more of your constant readers, who feel equally anxious on the subject with myself, as to the practicability of placing a conductor to a house in a street in London, without endangering, by the attraction of the electric fluid, the safety of the houses contiguous, and also whether you know of any persons who are in the habit of undertaking the erection of such conductors.

I remain, Gentlemen, your very humble servant,  
A CONSTANT READER.

A "Constant Reader" need be under no apprehension of endangering the neighbouring houses by erecting a paratonnerre on his own. He will observe in our translation of Gay-Lussac's article on that subject, in our last number, that paratonnerres are in general use in the large towns in America, and we believe there is no instance on record of mischief of the nature he apprehends, having been produced by them. When raised to a sufficient height above the *chimneys*, and furnished with *sharp copper points* (if well gilded so much the better), they may not only save a building if struck, by conveying the lightning in a harmless current to the ground, but also, for the reasons given in our last number, pp. 429 and 438, prevent the stroke altogether. The directions given by M. Gay-Lussac, both as to the mode of constructing the apparatus, and of fixing it, should be carefully attended to, especially observing that there must be no breaks in any part of the paratonnerre, and that it descend to a sufficient depth into a well of water, or ground that is constantly moist.

We are not acquainted with any artist who has paid particular attention to the subject, but from our knowledge of his abilities, and the excellence of all the philosophical apparatus that we have had occasion to employ, which has been made by him, we should recommend Mr. Newman, of Lisle-street, Leicester-square, as the fittest person we know to be employed on such an occasion.—C. and P.

## ARTICLE VIII.

*Reply to Dr. Fitton's Paper in the "Annals of Philosophy" for November, entitled "Inquiries respecting the Geological Relations of the Beds between the Chalk and the Purbeck Limestone in the South-east of England."* By T. Webster, Esq. Sec. G. S. (With a Plate.)

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

November 4, 1824.

IT was with some surprise that I read a paper by Dr. Fitton in the *Annals* for November, since it appears to point out as the present state of my knowledge some letters written by me 13 years ago on the subject of geology. Although my professional avocations ill admit of the sacrifice of time, yet I cannot, in justice to myself, pass by entirely unnoticed some observations in that paper; and should I appear tedious in my reply, I must crave the indulgence of your readers, since it is obvious, that a few words may depreciate, but that to remove the impression thus produced, many are often required.

Dr. Fitton observes, that the "geological relations of the beds of sand and clay, which are interposed between the chalk and the Purbeck limestone, have been of late the subject of considerable discussion:" but as he does not state what was the particular matter in dispute, and since the general reasoning in his paper rests upon his assuming that as decided which was the very thing discussed though not determined, it is not astonishing that he should have arrived at the conclusion, that the arrangement and names which I have adopted for the beds in the Isle of Wight are erroneous.

Perhaps this discussion should have been confined where it originated, among a few members of the Geological Society, until, by a more correct examination, than has been hitherto made, of the whole of this part of the series of English strata, the question should have been determined; yet (anxious only for the truth), I can have no objection to the tribunal before which it has been brought, well knowing it to be *always just*, when the *facts* are laid before it. Had I been previously acquainted with Dr. Fitton's intention of publishing on this subject, I might have been spared the duty of pointing out some misstatements respecting myself, which could have originated only in the haste which he appears to have been in.

As some apology, however, for thus occupying the public attention, I think it probable that this will ultimately prove useful to the cause of geology, by illustrating some points extremely important but hitherto obscure, and attended to by a few persons only.

Since, I believe, the work is but little known, which Dr. Fitton has described as a "standard publication which has been referred to by all geologists in treating on the Isle of Wight," and which has now become the subject of his criticism, it may be desirable that your readers should know something of its history.

English geology was, in 1811, only beginning to arrive at that advanced state for which it has been so much considered in every part of the civilized world. Many persons of distinguished abilities had occasionally bestowed their attention on a subject, than which none is more capable of exciting curiosity and enthusiasm; and although little parade was exhibited by men equally remarkable for their modesty as for their talents, yet the names of Woodward, Michell, Grew, Davy, Smith, Parkinson, and many others whose names it would be invidious to mention, will ever live in the history of the science. But that beautiful order which genius had begun to develope, and to separate from the almost chaotic state in which it had been hitherto concealed, was yet but imperfectly traced. No fixed principles of classification were established: no types of particular beds or formations were pointed out, to which all ought to refer; but every man who had, or fancied he had, a mind capable of arranging facts into a system, thought himself at liberty to make the attempt in his own way.

About this period it was, that my attention was accidentally led to the subject of geology. Having been originally educated as an architect, and much accustomed to the practice of drawing, I was fixed upon by the late Sir Henry Englefield to examine into a few points which had escaped his notice in the Isle of Wight, with the view of completing a work which he had composed on that island several years before, and which he was then preparing for the press.

It was not in the contemplation of Sir Henry Englefield that I should make a complete re-examination of the Isle of Wight; but, in order to accomplish the object which he had in view, I felt the necessity of looking more particularly into its stratification, and of applying to it some of the geological doctrines which were afloat at that time, but which had appeared since he had been a practical geologist; and I was in consequence led to view the subject very differently from him, and to develope the general structure of the island.

Sir Henry Englefield, with that liberality of mind which rendered him esteemed by all who knew him, expressed his satisfaction that I had exceeded the commission given to me, assisted me in making another journey to the coast of Dorsetshire for the purpose of extending my inquiries, and, on my return, far from wishing to appropriate to himself the information I had thus procured, resolved (as he has himself

state,†) to give it to the world in my own words,\* thus setting a noble example of that strict integrity and refined sense of honour which ever distinguishes the true patron.

Some of the observations which I had then made and reflected on, led me to perceive that I had discovered a *freshwater formation* which had hitherto been unknown to all English geologists: and I afterwards undertook a journey, at my own expence, for the purpose of studying, with all the attention that circumstances would allow me, a phenomenon so curious and unexpected. The result of this journey I communicated to the Geological Society, and it may be seen in the second volume of its Transactions.

Such has been the commencement of my geological pursuits; and if it had happened that this production became "a standard" for other geologists (an honour never aimed at or imagined by me), it would have proved that it was well thought of by my cotemporaries; but the table which Dr. Fitton has inserted in his paper will show, that my arrangement was *not adopted* "as the standard," but that the several geologists who have since visited the Isle of Wight had zeal, industry, and independence enough to look and think for themselves; and that if, in most cases, their observations agree with mine, it is either because both are right, or that such causes of obscurity existed, that we sometimes fell into the same errors.

It is well known that since the publication of my letters to Sir Henry Englefield, I have made several visits to the districts there described, for the purpose of examining them still more particularly, thus acquiring the knowledge of many facts that had originally escaped me. Many of these additional observations I have, sometime since, laid before the Geological Society,† and while Dr. Fitton's paper was in the press, I was preparing one which has since been read (see an abstract of it in the *Annals* for Dec. p. 465) on the same subject.

The discussion to which Dr. Fitton has alluded in the beginning of his paper, first made its public appearance in a work entitled "Outlines of the Geology of England and Wales, by the Rev. W. Conybeare, FRS. and MGS.; and W. Phillips, FLS. and MGS." and published in 1822. The passage, which may

\* The work here alluded to was published in 1816, and is entitled "A Description of the principal Picturesque Beauties, Antiquities, and Geological Phenomena, of the Isle of Wight, by Sir Henry Englefield, Bart.; with Additional Observations on the Strata of this Island, and their Continuation in the adjacent Part of Dorsetshire, by Thomas Webster, Esq.; illustrated by Maps and numerous Engravings by W. and G. Cooke, from Original Drawings by Sir H. Englefield and T. Webster." 4to.

† See a paper on the Reigate Stone, vol. v. Trans. Geol. Soc.

..... on a Freshwater Formation at Hordwell, vol. i. Second Series, Trans. Geol. Soc.

..... on the Cliffs at Hastings; not yet published: but of which an abstract appeared in the *Annals* for July, 1824.

be seen in the chapter "On the Beds between the Chalk and the Oolites," p. 150, is as follows:

"Having thus traced these formations uninterruptedly from the coast into Surrey, it will be our next object to describe their appearance on the east of that county, near Merstham and Reigate; and this we shall do somewhat more minutely, since, on the ground above stated,—namely, the continuous course of each formation from the coast, we feel ourselves compelled to dissent from the opinions advanced by a writer of whose eminent services to English geology one estimate only can be formed; and who, from the inspection of this single spot, has pronounced the *firestone beds*, which we assign to the *chalk marl* formation, to belong to that of the *green sand*, and the range which we consider as the *true green sand*, to be *iron sand*." This is followed by many details respecting these beds, from observations made by Mr. Phillips and himself, who, throughout their work, continue to employ the terms green and iron sand according to their own views as expressed above.

The paragraph by Mr. Conybeare, just quoted, was written in consequence of a paper which I had, a short time before, read before the Geological Society, "On the Geognostic Situation of the Reigate Stone." I there endeavoured to show, that the section of the country from Merstham to Nutfield is analogous to that of the beds below the chalk in the Isle of Wight; stating that the Reigate stone agreed with the Undercliff, and that the sand of Nutfield and Redhill below the fuller's earth pits was the ferruginous or iron sand.

The object of this paper was not to go into a close comparison between the beds in the two places; but any one who will consider with attention the table which I originally formed of these strata, in the work of Sir H. Englefield, and the following passage from my paper on the Reigate stone, may easily perceive what was my opinion on this subject:—

"On putting together all these circumstances, viz. the nature of the Reigate firestone, and its subordinate beds of chert and hard-zag, its situation below the chalk marl and above the ferruginous sand; and comparing it with the nature and situation of the green sandstone in other places, for instance, at the Undercliff in the Isle of Wight; the identity of these formations appears to me as evident as any with which I am acquainted; and whatever anomalies there may be in the history of the English strata, yet here, at least, no difficulty presents itself, but only such slight differences as every new locality exhibits."

In order that your readers may feel some interest in this question, which is really extremely curious in its nature, as well as important in English geology, I must explain, that the terms *true green sand* and *iron sand*, as used by Mr. Conybeare and myself, are intended *not merely mineralogically*, or as expressive

of the nature of the substances found in the places spoken of, but that they are meant to apply to certain strata or beds of England, which have been formerly observed, and so named by English geologists; the first from its containing abundance of dark green particles called green sand, and the second from having in it much iron ore.

These names, therefore, although originally given to the beds with reference to their obvious general characters, are, as far as this question is concerned, no more than A and B. A certain bed, immediately below the chalk, and containing much green sand or green earth, had been called *the green sand*, or A;\* and another bed, situated lower down in the series than the last, and containing much iron had been denominated *the ferruginous* or *iron sand*, or B. Now, two beds have been subsequently observed in another part of England; I have referred the upper one to A of former geologists, and the lower one to B; but Mr. Conybeare refers the lower one to A, and the upper one to another bed still higher in the series, viz. the chalk marl.

The propriety of these names have nothing to do with the question, nor the opinion of any geologists respecting another mode of nomenclature. They are mere names, by which we distinguish these beds from each other; and indeed they are highly expressive of the characters of the beds. Had Mr. Conybeare intended to state, that, in his opinion, the term green sand would be more properly applied to another bed than that which had hitherto received it, this would entirely change the view of the case; but he would then have alluded to the bed which had previously been so called, and have proposed the change. I do not think that this was his meaning, and I am certain that he is too candid to resort to such an explanation.

In giving names to the beds below the chalk in the Isle of Wight and the wealds of Surrey and Sussex, in 1811,† I followed what I considered to be the practice of that time. We had not then a geological map of England, and I called them *green* and *ferruginous* sand, as I thought they would have been named by geologists. I considered the rock of the Undercliff to be *the green sand*, having in my mind the vale of Pewsey and other places; the bed of clay below it I called *the blue marl*; and the whole series of beds below this I denominated *the ferruginous sand*. By the last I intended to express a group‡ of several

\* Several other beds of England have similar green particles as a part of their composition; as some parts of the London clay, plastic clay, the coites, &c. but we do not for that reason call them green sand.

† See the Table at the end of my letters to Sir Henry Englefield.

‡ This practice of arranging beds first into groups I had found necessary upon several occasions; and I was the first who divided the nine beds described by Cuvier and Brongniart in the basin of Paris into four groups, for the purpose of easier comparison with those above the chalk in the Isle of Wight, founding this arrangement upon the causes that operated during the formation of the strata. This practice has been continued, and is found to facilitate the study of the secondary beds.

beds of ferruginous sands and clays, which, having examined them not only in the Isle of Wight, but through an extensive tract in Dorsetshire, I found so connected together, that I was not then able to separate them from each other through the whole of that distance. The term *ferruginous* appeared to me not inaptly applied, on the first view of the subject, since the beds both *above* and *below* the weald clay contain in many parts a very large quantity of iron ore; and they have both, by one author or other, been called *the ferruginous sand*. The principles for the classification of beds, even in the present day, are not determined; and hence, in a great measure, the various opinions with respect to where the lines of the separation of groups should be drawn. At that time, when still less was known, it cannot be extraordinary that I should have been led to group together a set of beds possessing a common feature so remarkable.

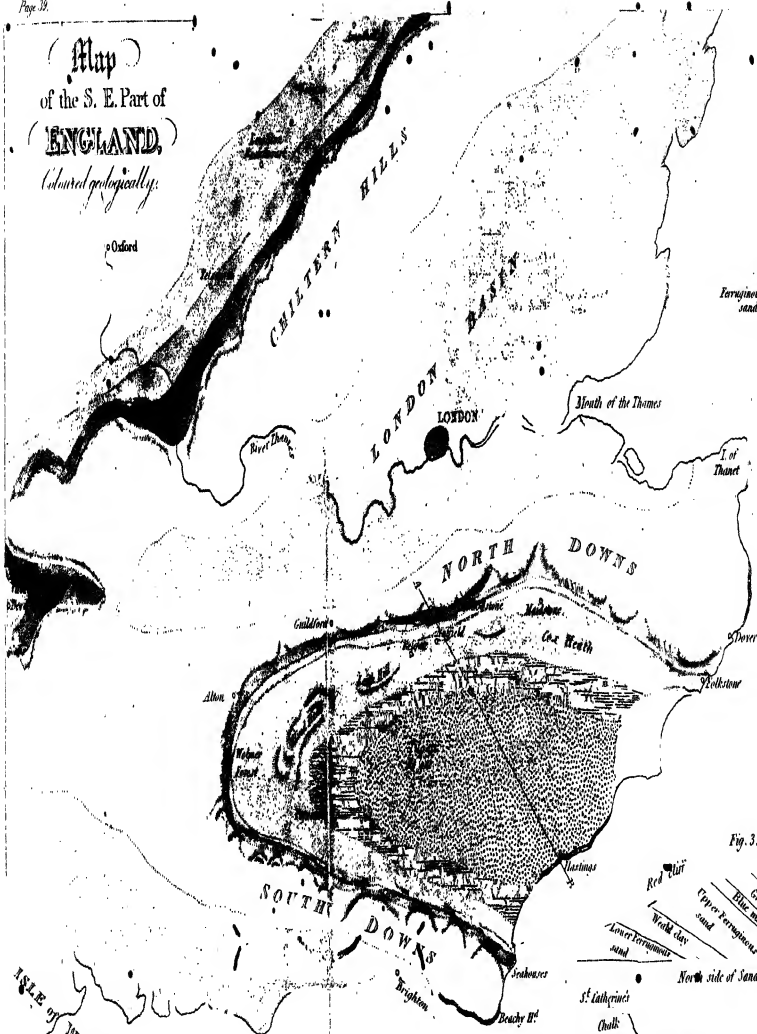
The following table will exhibit my arrangement of these beds in the south-east of England, together with the view which Mr. Conybeare took of the same subject:

Arrangement and Names of the Beds.		Places where the Beds are well seen.	
By Mr. Webster.	By Mr. Conybeare.	Isle of Wight.	Surrey, Kent, and Sussex.
Chalk.		Culver Cliff, in Sandown Bay.	Beachy Head. Folkestone. Merstham.
Chalk marl.	Chalk.	Sandown Bay. St. Catherine's Down.	Merstham. Guildford. Beachy Head.
Green sand.	.....	Sandown Bay. Undercliff.	Merstham. Godstone. Beachy Head.
Blue marl.	Chalk marl.	Sandown Bay. Undercliff.	Folkestone. Godstone. Merstham.
Upper ferruginous sand.	.....	Red Cliff, in Sandown Bay. Black Gang. Compton.	Nutfield. Wolmer Forest. Hind Head.
Weald clay.	Green sand.		
	.....	Sandown Bay. Cowleeze Chine.	Berthersden and Marsden, in Kent.
Lower ferruginous sand.	Ferruginous sand.	Sandown Bay. Cowleeze Chine. Brookpoint.	Hastings.

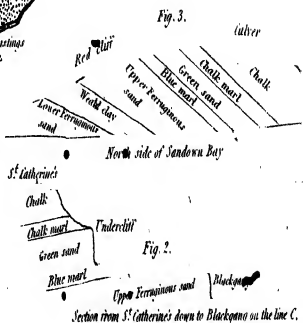
To show, in part, the evidence from which I have deduced the order and arrangement of the strata, I have represented in Plate XXXV. fig. 1, a section across the counties of Surrey and Sussex, from Merstham to Hastings. Fig. 2 is a section taken from the top of St. Catherine's Down, Isle of Wight, to the bottom of Black Gang Chine; and fig. 3 exhibits a very satisfactory and instructive section which is seen on the north side of Sandown

(Map)  
of the S. E. Part of  
(ENGLAND)  
Coloured geologically.

o Oxford



Drawn by The Author





Bay, which is perfectly accessible, and which from the remarkable and highly inclined position of the beds, is one of the very best situations for examining the beds immediately below the chalk.

The map in Plate XXXV is a slight sketch of the SE. of England coloured geologically, according to my original view of the subject.

Dr. Fitton has inserted in his paper, (p. 369, of the *Annals* for November,) a table of these strata, in which he gives the same name as Mr. Conybeare to one of the principal beds, viz. that which he calls *green sand*, and which was a part of my *ferruginous sand* group; but he does not stop to discuss this question; and by his continuing to call the bed by the name of *green sand* throughout his paper, I must suppose that he has adopted Mr. Conybeare's view of the subject.

In order to bring this matter to an issue, it appears to me that we ought first to inquire what were the beds originally designated by the terms *the green sand* and *the ferruginous sand*.

I am not able at present to say by whom the term *green sand* was first employed as a name for one of the strata of England; but that is not necessary. It is sufficient if I point out that it had been long in use by the geologists who preceded us.

The term *green sand* is to be found in the writings of Mr. William Smith, who, as will be seen by an able sketch of the history of English geology in the *Edinburgh Review* for 1818, and by Mr. Conybeare's introduction to his "Outlines," above-mentioned, has the strongest claims on our gratitude, and who is indeed (I had almost said) the father of modern English geology.

Another author of great authority, by whom this name has been employed for a long time previous to this discussion, is the late venerable rector of Pewsey, the Rev. Joseph Townsend, in his work, entitled "The character of Moses established for veracity as an historian." In the preface to this work, which is replete with the most valuable facts, he informs us, that he was indebted for his knowledge of the succession of the beds of England to Mr. William Smith; but as he was himself a most assiduous practical geologist, we may consider his account of them as the descriptions of Mr. Smith, verified and extended by himself.

It appears to me that from these and similar sources, our first ideas respecting the green and iron sands have been derived, either directly or indirectly.

In the writings of these authors, it will be found that every where on the west escarpment of the chalk, which passes through Norfolk, Bedfordshire, Oxfordshire, Wiltshire, &c. the beds immediately below it are seen in succession; and that they

have described them, beginning with that next to the chalk, as green sand, blue clay or marl, and a red or ferruginous sand. Of the first, we have a remarkably fine example in the vale of Pewsey, celebrated for its fossils; and the latter (the ferruginous) is well seen at Woburn and Leighton Beaudesert. These I consider as undoubted *types* of the formations in question, being described under these names by all geologists to the present day, in the tables and maps which have been published. It will, therefore, simplify this discussion if we attend to two points.

1. Whether the bed which I have called *green sand*, viz. the Undercliff, Isle of Wight, and the Reigate stone, be the same bed with that so called by Smith and Townsend on the west of the chalk, and particularly in the vale of Pewsey?

2. Do the Woburn and Leighton Beaudesert sands agree with that bed in the wealds of Surrey, Kent, and Sussex, which is between the Folkstone blue marl and the weald clay, and which is seen at Cox Heath, Nutfield, Wolmer Forest, &c.; or with the Hastings beds in Sussex, which are *below* the weald clay?

With respect to the first, I might observe, that the Undercliff has been actually called green sand not only by me, but by Mr. Conybeare in his "Outlines;" by Prof. Buckland in his Table of the English Strata printed for distribution; by Prof. Sedgwick; in short by all English geologists who have attached the same name to the bed in the vale of Pewsey; and I might show that Dr. Fitton in his paper admits what I have before endeavoured to prove, that the Undercliff is identical with the Reigate stone. The thing, therefore, seems to be done. Pewsey = Undercliff by Mr. Conybeare; Reigate = Undercliff by Dr. Fitton; and hence, since things equal to the same are equal to each other, Pewsey = Reigate. Q. E. D. But I will not take this advantage; since Dr. Fitton has done me the honour to hint that these gentlemen followed me with respect to Undercliff being = Pewsey, a circumstance which I never heard of before it was rumoured that I was wrong.\*

I find that Mr. W. Smith, in the memoir which accompanies his geological map of England, applies the term green sand to the bed *immediately below* the chalk, and *above* the blue marl, or oak tree clay. This blue marl he identifies in his map with the Tetsworth clay, and with the gault, the latter being admitted by all to be the same as the Folkstone clay. It appears to me to be clear, therefore, that the *green sand* of Smith could not be

\* Mr. Conybeare states, ("Outlines," p. 120,) that "in 1813 he made a tour in the Isle of Wight and in Purbeck, and formed detailed lists of the several strata constituting the series as exhibited in the various points where their sections are exposed in that interesting district."

the Coxheath, Nutfield, and Wolmer Forest range which is *below* the Folkstone blue marl. But as a decisive proof of this, he states, that the bed below the blue marl is *the Kentish rag*, which with him is a different bed from the green sand.\* It is to be regretted, that the writings of Mr. Smith are rendered obscure when describing these beds, by his considering the Purbeck stone and the Kentish rag to be the same; and he even sometimes places the Portland stone in this part of the series. But it does not appear that this geologist was acquainted with the latter beds *in situ*; nor that their true place was understood previously to my examination of these countries; the first table in which they were correctly placed being that in the commencement of my paper on the Freshwater Formations of the Isle of Wight, vol. ii. Trans. Geol. Soc.; and the author of the review of Smith's Map (Edinb. Review, 1818, p. 32), seems to have been aware of this circumstance.

The *ferruginous sand* of Smith is evidently the Coxheath and Nutfield range, which he identifies with the Woburn sands; and in his map he places his Kentish rag properly *in* this bed, although (perhaps through inadvertence) he arranges the description as if it were *above*.

Mr. Townsend describes the sands immediately under the chalk as of three varieties. 1. Green sand; 2. Grey sand; 3. Red sand (*arena ferruginosa*, Lin.) The first, or uppermost, forms the vales of Pewsey and Warminster, and is a sand containing dark green particles. The second is a calcareo-siliceous sandstone, called firestone, which he considers as identical with the firestone of Reigate, and the whetstone pits of Blackdown. As he does not mention any clay between these two (and it is remarkable that his green sand is not *under*, but *over* his grey sand or firestone), I imagine that they form one mass, and differ only in the quantity of green earth which they contain; for the firestone of Reigate is never quite without this ingredient, and in the Isle of Wight the upper part of the Undercliff contains more green earth than the lower. The occurrence of the firestone in the green sand of the vale of Pewsey is important, for nothing like this is found in the Coxheath and Nutfield range called by Mr. Conybeare and Dr. Fitton *the green sand*. The fossils of the vale of Pewsey, agreeing in general with what we

\* I was not originally aware of this circumstance. My letters to Sir H. Eglesfield were published several years before Mr. Smith's memoir; and not having had an opportunity at that time of examining Folkstone personally, but having heard the Folkstone blue marl described as the chalk marl, and the Folkstone rock as the green sand; and knowing from hand specimens that the Kentish rag is almost identical in appearance with the rag of the Undercliff, I imagined erroneously the Folkstone rock to be a part of the same range with the Reigate stone, and consequently to lie decidedly *above* my ferruginous sand; nor was I undeceived until by a visit which I made to Folkstone in the summer of 1822, I found that the Folkstone rock lay immediately upon the weald clay without the intervention of any ferruginous sand.

know of those of the Undercliff, afford an additional evidence in favour of the identity of these beds.

We have another section of the strata below the chalk, described in Mr. Conybeare's "Outlines," p. 162. He states that in the parish of Roak, in Oxfordshire, a stone lies under the chalk, and is worked for building. The fossils found in it (hamites, turrilites, inocerami, scaphites, and ammonites) agree with those of the Undercliff; and it stands upon the Tetsworth clay, which again rests upon an iron sand.

The stone of Totternhoe, in Bedfordshire, and of Reach, in Cambridgeshire, is soft, immediately below the chalk, and is similar to that of Roak and Reigate.

I have already mentioned my original opinion, that the structure of the weald and that of the Isle of Wight are the same, a circumstance that it was impossible to comprehend upon the supposition of Mr. Conybeare; but my late discovery of the Hastings limestone in that island, having enabled me to speak with more confidence on the subject, as affording me a fixed point in each of these places (see the abstract of my paper on the subject, together with the table of the equivalent beds in the two places in the *Annals* for Dec. p. 465), the identity of the rock of the Undercliff with the Reigate stone is no longer doubted, and is admitted by Dr. Fitton. The rock of the Undercliff, Isle of Wight, is immediately below the chalk marl, and has the general characters of the Reigate stone, although its thickness is much more considerable, and its beds of chert and hard limestone are in proportion more striking. It rests upon a bed of blue marl, and that again upon a ferruginous sand.

Thus we see that almost every where below the chalk in England (or rather below the chalk marl according to my arrangement), there is a stone composed of siliceous grains, mica, and dark green particles, with a calcareous cement. In some places it is very hard; in others soft, and fit to be employed as firestone; and in others again too soft for this purpose, and scarcely distinguishable from the common chalk marl, into which it sometimes passes. This stone also varies much in colour, chiefly from the greater or less proportion of green particles dispersed through it. Its fossils are also very unequally distributed; those which are the most characteristic of the bed are inocerami, hamites, turrilites, trochi, alcyonites, ammonites, &c. It is to this bed, as appears to me, that the name of *green sand* was originally given by English geologists; and from the above and similar observations, I conclude that the green sand bed in the vale of Pewsey is the same with the rock of the Undercliff, Isle of Wight, and with the Reigate stone.

Let us now attend to the second question; viz. whether the Woburn sands belong to the Nutfield range, or to the Hastings beds?

I shall begin by stating, that a bed of ferruginous sand appears at Hunstanton, in Norfolk; and passing under the marshes of Ely, reappears in Huntingdonshire; is seen in Bedfordshire under the name of the Woburn and Leighton Beaudesert sands, and extends into Oxfordshire. This bed has been called by all English geologists, to the present day, *the ferruginous sand*; and is so represented in all the geological maps. It is the third in succession below the chalk, being separated from it by one of firestone (the green sand of Smith), and another of blue clay or marl.

In the wealds of Kent and Surrey, a bed extends from Folkstone through Coxheath, Leith Hill, Nutfield, Wolmer Forest, Hindhead, &c. but sinks below the sea at Beachy Head. In the greater part of its course, this bed is highly ferruginous; but in some places, and remarkably from Folkstone to Maidstone, it has in it beds and nodules of hard limestone with dark green and ferruginous grains. The fossils are much fewer, and do not appear to be the same as those in the bed above it which I have called green sand.

In the Isle of Wight, also, a bed of highly ferruginous sand lies in the same situation with respect to the chalk, at Red Cliff in Sandown Bay; and it may be traced along the south side of the island by Shanklin, Dunnose, Blackgang, Atherfield, and Compton; and also over a great part of the interior of the island. This sand, which varies much in its character, contains in many parts abundance of granular hematitic iron ore, and sometimes green earth. It also has, in some places, though not generally, nodules of limestone with fossils and green particles which resemble the rock of Folkstone.

Although the Car stone and Woburn sands have been regarded by Mr. Conybeare as identical with the Hastings beds, and not with the Nutfield range, yet he has not favoured us with his reasons for this opinion; and no sections have been published that prove it. On the contrary, when we examine the succession of beds as described by Mr. Smith and others on the west of the chalk in England, we find that it resembles exactly the structure of Kent and Surrey, the succession being green sand, blue marl, ferruginous sand. I may add, that on examining hand specimens from the bed below the gault in Cambridge, they appear to me to resemble exactly those from Red Cliff, Isle of Wight, which is allowed by Dr. Fitton to be the same as the Nutfield range.

In all the above-mentioned places, this bed is separated from the last (the green sand of Smith and Townsend) by one of blue clay or marl, which in Cambridgeshire is called gault, at Folkstone the Folkstone blue marl, and in the Isle of Wight, by myself, the blue marl. This bed is generally characterised by peculiar fossils, although these are very unequally distributed.

Those of the gault and the Folkstone marl are numerous and identical, while in the Isle of Wight they are extremely few. Mr. Smith has named this bed sometimes the Oak tree clay, and sometimes he calls it Brick earth. He describes it in his work on Organized Fossils, published in 1817, p. 36, as existing at Godstone; to the north of Reigate, under the Reigate stone; at Leighton Beaudesert, Bedfordshire; at Grimston, in Norfolk; and at Westering, in Bedford, four miles SW of Ampthill. It is in all these places distinguished by its characteristic fossil, the small fusiform belemnite, and contains many other fossils of this bed, as hamites, inocerami, depressed ammonites, &c. This blue marl is identified by Smith with the Tetsworth clay, notwithstanding the latter has been considered to be the weald clay by Prof. Buckland, in his table of the order of the strata. The brick earth mentioned by Townsend above the red sand at Devizes is probably this bed, and I have little doubt but that it may be traced all round the west side of the chalk as Smith has represented it in his map.

From the above considerations, I am still inclined to think, that the bed extending through Coxheath, Nutfield, &c. belongs to the *ferruginous sand* of former geologists, and not to their green sand.

With respect to the ferruginous sand *below* the weald clay, the Hastings beds, we have nothing satisfactory to prove that it exists on the west of the chalk. It has not been stated any where (as far as I know), that the ferruginous sands in Bedfordshire contain any fossil shells, or this question might be decided, since the fossils of the Hastings beds are peculiar, and supposed by some to be chiefly, if not entirely, of freshwater origin, whereas those of the Nutfield range are marine. Nevertheless, it may yet be found in that quarter, although we have no evidence before us from which we can draw any conclusion; and it is quite surprising to find, that, at a time when some imagine the geological account of England to be nearly complete, we should be absolutely in want of materials for determining so important a question.

Having now arrived at a certain point in this discussion, viz. the determination of what were the original *green* and *ferruginous* sands, and having stated my reasons for giving these names to the beds in the Isle of Wight, and in the wealds of Surrey, Kent, and Sussex, it is natural that I should inquire how it could be that the bed which I imagine to be the *ferruginous sand* of Smith, came to be called the *true green sand* by Mr. Conybeare, and several other geologists of the present day. I think I perceive the solution of this in Mr. Conybeare's work itself, the "Outlines." He begins his examination of the beds below the chalk at Folkstone; and finding there a dark coloured argillaceous bed immediately below the chalk, he calls it (though inaccurately) the chalk marl,

and the calcareous rocky bed below this the green sand, the latter bed resembling, indeed, in many particulars, the rock of the Undercliff, Isle of Wight. It happens that the bed of Reigate stone, which continues its course eastwards past Godstone, appears not to reach to Maidstone, and is actually wanting at Folkstone, at least in the form of firestone: thus the chalk or chalk marl is divided from the hard rock of Folkstone only by a bed of blue marl; and since the true chalk marl is itself frequently grey, and as the hard rock below the blue marl resembles some of the states of the original green sand, it is not at all surprising that this error should have been fallen into.\* We have no method of determining upon the identity of beds in places distant from each other, but by the correspondence in their nature and their order of superposition: there appeared, therefore, considerable reasons for determining the Folkstone rock to be the green sand. This being supposed to be established, it followed necessarily, that although the rest of this latter range, from Coxheath all round the weald, varied almost entirely in its character, so as to be a *ferruginous* sand (mineralogically); yet (since according to the principles of geognostic nomenclature, beds do not change their names in different places according to the qualities of the substances forming them,) the *whole bed* acquired the name of *green sand* which had thus been given to a part. This is my view of the way in which I conceive so good an observer as Mr. Conybeare might have been led to give to a stratum a name which did not properly belong to it. I have thus stated frankly my opinion on this subject; and it will remain for him to say how far I am right. It is, I am aware, putting his candour to a severe test; but in that I have the fullest confidence.

I must now return to Dr. Fitton. In p. 366, line 12, he observes, that Mr. Conybeare “adopted my arrangement of the strata of the Isle of Wight, and regarded the lower part of that island as composed of *one series only* of ferruginous sand which he identifies with those of Hastings.” He then proceeds to show, in p. 367, that there are *two* distinct series of sands below what I have called green sand, which are separated from each other by a stratum of clay. In this passage I am under the necessity of pointing out a double oversight; for, in the first place, he must have been aware that I had two ferruginous sands in my arrangement, since he actually states it in his table, p. 369. In the second place, he might have seen that Mr. Conybeare does *not adopt* my arrangement, but distinctly confines *his* ferruginous sand to my *lower one only*.

Dr. Fitton has also stated, erroneously, that I mention the Purbeck beds as existing in the Isle of Wight. Since I had never expressed this decidedly, and as I had long known that they are not to be found there (having several years ago ex-

mined the isle of Purbeck, and presented to the Geological Society a complete set of specimens of all the beds), I could not conceive what had led to this mistake, and turned over my letters to Sir H. Englefield in search of any passage that could bear such a construction. The only one that appears to be the least obscure is that where I mention (p. 122) the thin layers with shells in a clay (the weald clay), in Sandown Bay, called Platten, respecting which I observe, that "they *much resemble* Purbeck stone, *but the shells are larger.*" It must be obvious, that by this I mean only that the two kinds of stone have much the same appearance, without any attempt to identify the beds from which they came. I had not at that time seen the Purbeck stone *in situ*, and spoke merely of it as knowing it in building. Indeed that no other inference can be fairly drawn from my expression is evident, since Dr. Fitton himself says, p. 374, that "the limestone of the weald bears altogether a striking resemblance to the Purbeck limestone;" and yet he does not intend to express they are the same. My having mentioned this resemblance shows that at a very early period of my investigation, I was struck with an analogy between the Platten in the Isle of Wight, and the Purbeck and Petworth marbles, an analogy which I have since extended.

Dr. Fitton is perfectly correct in stating, that, at the time when I wrote those letters to Sir H. Englefield, I had not duly appreciated the importance of the weald clay. The fact is, it is far less conspicuous in the Isle of Wight as a valley separating two ranges of high ground than in Kent and Sussex; nor, at that early period, had the difference between the fossils of the Hastings beds and the Folkestone rock been noticed. But I had been gradually approaching, and had finally arrived at the same conclusion as Dr. Fitton has now done.

I mentioned in my paper on the Freshwater Formations of the Isle of Wight, and also in one of my letters to Sir H. Englefield,\* the probability, that part, at least, of the Purbeck series was of freshwater origin; and I possess specimens which I brought with me from the Purbeck beds on my first visit to them in 1812, containing several species of freshwater shells converted into calcedony, but mixed with others that are marine. I also stated in my table of the strata, that the Petworth marble might perhaps belong to the same series, from the analogy in its fossils. The univalve shells of the Petworth marble are in general larger than those of the Purbeck, but I have since found beds

\* The passage is as follows:—"It was long ago observed by Woodward, in his History of Fossils, that the shells in the Purbeck marble consisted chiefly of the *helix vivipara*; and it is rather surprising that this very ancient freshwater formation should not have excited more attention." Beautiful impressions of fish are frequently met with by the quarrymen between the laminæ of the limestone; and I saw abundance of fragments of bones, some of which belonged to the turtle. Complete fossil turtle have been found, and lately one extremely perfect." (Letter 9, p. 192.)

in the isle of Purbeck with shells quite as large as those of Petworth, and apparently of the same species. In my examination of the Hastings beds, I remarked the resemblance of the casts of the univalves to those of the Purbeck and Petworth marbles, and that the fossil shells were altogether different from those of the green sand.

I always considered the weald clay as intimately connected with the Hastings beds, and with the Purbeck stone, from the analogy in the fossils,\* and from the resemblance between the Platten in the Isle of Wight, the Battle beds, and the Purbeck beds, although I could not then determine to what part of the series each of these should be referred. This led me originally to class the weald clay as a subordinate bed of the Hastings ferruginous sand, which contains several other beds of clay, although that of the weald is the most considerable; and this arrangement I still adhere to. In my first examination of the Isle of Wight, the fossils of Shanklin escaped me; and I owe my knowledge of them to Prof. Sedgewick's valuable paper on that island in the *Annals* for May, 1822. Since that time I have perceived the necessity for separating the upper from the lower ferruginous sands.

I agree with Dr. Fitton, therefore, in the propriety of making the separation between the two sands at the top of the weald clay; but I think I see zoological reasons why the latter ought not to be called a distinct formation, but that it should be formed into a group with the Hastings and the Purbeck beds. I do not go so far however as to consider these as freshwater formations, a term which I am accustomed to restrict to such beds only as have been probably formed in freshwater lakes.

I come now to consider the changes which Dr. Fitton has proposed to make in the names of the beds which have been treated of.

With respect to the proposal to change the name of the rock of the Undercliff from green sand to *firestone*, I am compelled entirely to dissent from it. It has already been called green sand by all geologists; no arguments have yet shown that it is not entitled to that appellation, and mere change is obviously worse than useless. Firestone is a term used by builders to express a stone of a certain quality, that of resisting the fire, and which is employed for hearths and covings to chimneys; this name can, therefore, be applied with propriety only to a stone having that property. As only a certain portion of this bed, and that only in a few localities, is fit for such a purpose,

\* The small organic body supposed to be a cypris, I found in the weald clay in Sandown Bay, when on a visit to the Isle of Wight, in 1819, with Mr. Broöke, together with paludinae and the teeth of fish. I also pointed out to Dr. Fitton the resemblance of the Hollington limestone (first observed by him in that locality) to the freshwater rock at East Cowes.

the use of this term, applied indiscriminately to all parts of the bed, would lead to much confusion when it came to be used for economical purposes.

The appellation of the fourth in Dr. Fitton's list (my upper ferruginous sand) is more difficult to agree upon, since various opinions have been entertained respecting it. If my view of the subject be ultimately found to be correct, that is, if this bed be found to agree with the *ferruginous sand* of the west, it would seem right that it should retain its original name; for to call the Carstone of Norfolk and the Woburn sand, &c. the *true and only green sand*, would be such a preversion of names that it could not be tolerated: and should the rock immediately under the chalk in the vale of Pewsey really prove, by a correct examination, to be the same as the Undercliff and the Reigate stone, what would be the consequence if we adopt Dr. Fitton's nomenclature? The answer is obvious: it also must be called firestone; that is, the name *green sand* must be taken from the bed to which it originally belonged, to be attached to another which received it only through an oversight. I do not wish to insist that this has been the case; but at least the contrary has not been shown; so far, therefore, Dr. Fitton's decision is premature. However, as it has been called green sand by some eminent geologists; and since indeed it contains, in some places, a great quantity of the mineral from which this name has been derived; I have proposed, in a paper lately read before the Geological Society, to style it the *lower green sand*, or (to compromise the matter,) as I proposed before, in my paper on Hastings, *ferrugino-green sand*; the Undercliff being called the *upper green sand*. By this arrangement, a group will be formed, which may be called the *green sand formation*, consisting of the upper and lower green sands; and the blue-marl between them will be the *marl of the green sand*. This marl has indeed considerable analogies in its fossils with the bed above it, into which it sometimes passes.

The *Hastings beds* may continue to be described by that name, until more is known; and the term ferruginous sand hitherto given to it may be relinquished, as that has been applied to the Woburn sands.

With respect to that part of Dr. Fitton's paper (p. 367 and 383), where he appears to dissent from the opinions that have been stated on the subject of beds being more irregular than has usually been supposed, arising partly from the want of continuity in some, and a difference in the structure and composition of others, I shall only observe, that the subject on which his own paper and the present treats furnishes ample proofs, that the difficulties of identifying beds have been frequently underrated by geologists, from their not sufficiently attending to these circumstances. At first, in studying the secondary beds, the

newly discovered analogies occupy our attention most, as being infinitely the most interesting; but afterwards, our eyes are opened to the discrepancies, and these are also worthy of our notice, as it is from the "facts alone" that we can draw rational conjectures respecting the mode in which the strata have been formed.

I have now, I trust, prepared your readers for appreciating Dr. Fitton's remark, that I had ventured to make an arrangement of the strata of the Isle of Wight "without sufficient examination." It must be obvious to all experienced geologists, that the character of a formation, or series of beds, should not be drawn from any one spot, except that spot should contain the whole series: any character drawn from an imperfect part of the series must be liable to be corrected, when more is known. But who can say that he is acquainted with the whole series of beds in the great European basin, of which those of England are certainly but a part? and yet to delay making an arrangement of the British strata until the rest of Europe should be accurately examined, would be to neglect one of the very means by which we hope to arrive at the truth. To heap facts upon facts, without endeavouring to arrange them, would have excited no interest. An attempt, therefore, at an arrangement, has been made even at an early period in the progress of investigation, trusting that our successors, in making additions to the science, would duly appreciate our zeal, take into their consideration the progressive nature of knowledge, and correct our errors with a *gentle hand*. After all, what is sufficient examination? To some this question may appear to be easily answered; but when we reflect upon the changes that are perpetually making in the systems and arrangements of natural history from the discovery of new facts, we must soon perceive its difficulty. Few districts of England have been more frequently visited and examined by geologists than that in question; and yet it appears not to be exhausted.

With respect to myself, I can truly say that I have omitted no opportunity, that my very limited means have allowed me, to extend my inquiries, not only in this island, but on the opposite side of the Channel. Since the period of my letters to Sir H. Englefield, I have visited and examined, at my own expence, the neighbourhood of Paris, in order to see if my conjectures were well founded with respect to the analogies which I supposed to exist between the upper beds of England and France; and I had, in the summer of 1823, in a tour over the ground we have been examining, accompanied M. Brochant, Professor of Mineralogy in the École des Mines at Paris, who, with two of the élèves of that establishment, had been sent by the French Government to visit this country, and to verify the observations

which have been made by the geologists of England. As this visit by M. Brochant may at some future time be important in the history of geology, I think it may be useful here to insert an extract of a letter which I lately received from him.

\*\*\*\*\* "Une de mes plus grandes occupations, depuis deux mois, a été de ranger et d'étiqueter nos récoltes géologiques de l'année dernière. MM. Dufresnoy et Elie de Beaumont ont employé d'abord beaucoup de tems à mettre chaque chose en place : nous n'avons aucune confusion, au moyen de notre catalogue et de notre journal, et de toutes les précautions minutieuses que vous nous avez vu prendre. Cela m'a fait refaire cet été tout mon voyage d'Angleterre avec une vive satisfaction. J'ai revu cette charmante Isle de Wight, où vous nous avez fait si bien voir tant de coupes géologiques ; et j'ai pensé à cette aventureuse navigation pour doubler le cap de Handfast. C'est dans cette revue générale de nos récoltes, que nous avons reconnu, plus que jamais, combien nous vous étions redevables. J'ai fait, depuis 30 ans, bien d'autres tournées géologiques : aucune ne m'a été à beaucoup près aussi productive, n'ayant pas, comme en Angleterre, de savans guides pour me conduire sur les points caractéristiques, et m'en faire voir les rapports, ce que je n'aurais pu découvrir que par un séjour prolongé. Je suis étonné moi-même, tout en pensant aux facilités de tout genre que vous et d'autres savans avez eu la complaisance de me procurer, d'avoir pu faire tant en une seule campagne. Car je vous assure, qu'à l'exception des fossiles, dont il nous manque un grand nombre, nous avons une suite très belle, et presque complète, de toutes les formations géologiques de l'Angleterre. Avant la fin du mois d'Août, celle de l'Ecole des Mines sera livrée aux yeux du public ; et j'espère bien qu'elle contribuera à faire bien des rapprochemens entre nos terrains et les vôtres." \*\*\*\*\*

I am, Gentlemen, very truly yours,

THOMAS WEBSTER.

### *Additions to the Reply to Dr. Fitton.*

GENTLEMEN,

Dec. 3, 1824.

I am happy to find that the paper which I read before a meeting of the Geological Society on Nov. 5 (see an abstract in the *Annals* of last month, p. 465), containing, in other words, the substance of the above reply, has not been without its effect, since I perceive that Dr. Fitton has, since that time, in his "Additions," given up the term *firestone* upon the ground which I stated to the Society.

With respect to the term *green sand*, Dr. Fitton now observes, that "the misapplication of the term has really been the source

of so much confusion, that it seems better to give it up altogether (also), and to choose for the beds in question names entirely new." But is this quite necessary? and what will be the confusion in all the books already written and to be written, should we adopt instead of it the proposed name of *Shanklin sands*? For my part, I cannot help thinking that we ought to retain the term *green sand*: it has become almost national, and is endeared to us by many associations: it has been the frequent companion of our travels, and passes current on the Continent even without translation. Shall we discard an old friend because some one has misnamed him? or is this any thing like the classical practice of covering the combatants with a cloud to prevent the decision of a contest? Seriously, when will these changes end? or is every month's *Annals* to produce a new geological nomenclature? I really thought I had given up enough (considering the state of the question), when I proposed to turn my "ferruginous" into "lower green," not to save my own credit, but that of others.

In the same communication to the Geological Society, I suggested (I verily believe before any other person thought of it) that the Woburn sands agreed with what was considered by some as the *true green*; and I find by Dr. Fitton's "Additions," that he has since been consulting his maps, and that he has now come very near indeed to adopt my opinion on this subject. I am pleased also to find that Mr. Lyell has confirmed the observations which I made on the green sand at Beachy Head in 1813 (see my paper on the Freshwater Formations, vol. ii. Trans. Geol. Soc. p. 192), and that he observed a section at Spere which is the counterpart of that which I described before at Merstham (vol. v. p. 353). It is also satisfactory to perceive, that this gentleman has now arrived at the same conclusions that I had come to several years before with respect to the correspondence between the general structure of the weald and the Isle of Wight, and which had not been doubted until lately.

I shall be glad to avail myself of the extensive circulation of the *Annals* to state, that I will feel obliged to any gentleman, whose local knowledge of the districts on the outcrop or basset of the chalk in England, or on the Continent, may give him the means of examining them, for any information respecting the beds which appear immediately *below the chalk*, since it is my wish to pursue this interesting part of English geology, until the obscurities complained of shall be completely cleared up.

I am, Gentlemen, very truly yours,

20, Bedford-street, Covent-garden, London.

THOMAS WEBSTER.

## ARTICLE IX.

*On the Discovery of Selenium in the Sulphuric Acid made from the Pyrites of Anglesey.* By Edmund P. Thomson, Esq.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

Manchester, Dec. 16, 1824.

I BEG to send for your particular notice and examination, a substance that has lately come under my observation, and which, from the examination I have given it, I have no doubt is selenium, the substance discovered by Berzelius in the sulphur extracted from pyrites at Fahlun, in Sweden, an account of which will be found in the *Annals of Philosophy*, vols. xiii. xiv. xv. The presence of this new body, in one of the operations at my manufactory, will not be surprising, when it is known that in making muriatic acid I use sulphuric acid which is prepared from pyrites, at the works of my friend Mr. Robert Mutrie of this town.

The Selenium distils over with the muriatic acid into the receivers, and in the course of two or three days, it falls to the bottom of the vessels in the form of a reddish brown substance, which does not appear to deteriorate the acid in the least. The quantity of this new substance produced from 100 parts of the sulphuric acid (made from the pyrites) I have not yet been able to ascertain, but have reason to suppose it to be very small. Through the kindness of Mr. Robert Mutrie, I am enabled to furnish you with specimens of the pyrites used at his manufactory.

The pyrites is obtained from the Paris Mountain in the island of Anglesey, and there are two or three descriptions of them, but all from the same mountain.

I remain, Gentlemen, yours most respectfully,

EDMUND P. THOMSON.

*Experiments on the above described Selenium.* By J. G. Children, FRS. &c.

I have submitted the red substance forwarded to us by the kindness of Mr. Thomson, to a few experiments, in order to obtain unequivocal evidence of its containing selenium.

A fragment, heated on a slip of platina foil by the spirit lamp, tinged the flame of a beautiful azure blue colour. A portion heated by the spirit lamp in a glass tube closed at one end, gave off first acidulous water; some sulphur next sublimed and condensed at a little distance from the flame, and soon after a red substance, which condensed on the sides of the tube between the flame and the sulphur, and very near the former. During the sublimation of the red matter, the lower

part of the tube was filled with a yellow vapour, a good deal like chlorine, but of a deeper colour, and an unpleasant odour was exhaled, very similar to that of cabbage water. After the whole of the volatile matter had been sublimed, a fixed dark coloured residuum remained at the bottom of the tube. This was transferred to another tube, open at both ends, and again heated; some more of the red sublimate was thus obtained, and the residuum assumed a grey colour. It amounted to about 53 per cent. of the weight of the substance operated on, and on examination was found to consist of earthy matter, principally silica and lime; consequently the assay contains about 47 per cent. of volatile matters, by far the greatest portion of which consists of the red sublimate. The red sublimate had evidently been fused and spread over the inner surface of the tube.

When detached from the tube, a morsel of it imparted the same beautiful blue colour to flame that has been already mentioned, but more intense.

Another fragment, heated in a tube open at both ends, sublimed without giving off any sulphur, exhaling at the same time a strong odour similar to that of horse-radish. It fused very readily on being gently heated in a close tube over the lamp, and remained for some time in a soft pasty state.

These experiments are quite sufficient to establish the identity of our red sublimate with selenium, and in external characters also it perfectly answers the description of that substance. It has a metallic lustre, and a deep brown colour when seen by reflected light. Its fracture is conchoidal, and has a vitreous lustre. It is easily scratched by the knife; is brittle, and its powder has a deep red colour; but it adheres together readily when rubbed in the mortar, and then assumes a grey colour, and a smooth and somewhat metallic surface. In very thin laminae it is transparent, and when viewed by transmitted light, has a beautiful cinnabar red colour.

## ARTICLE X.

*Observations upon Mr. Whipple's Answer.* By R. Phillips, FRS. L. and E. &c.

IN the seventh volume of the *Annals of Philosophy*, p. 450, N. S. I offered some remarks upon the *Pharmacopœia Londinensis*, lately published by the College of Physicians; and in the last number of the *Annals*, I am requested by a correspondent who subscribes himself "G. Whipple," to give him an explanation of them. The manner of Mr. Whipple's communication is such as would have prevented its appearance, if any one but myself had been the object of it. I shall, however, show, that he may at

least congratulate himself upon his consistency, for his matter is worthy of his style.

The part of Mr. Whipple's letter which I shall first notice is the following: "I should esteem it an obligation, if favoured with a translation of the first nineteen lines of the paper, the *parvum in multo*." These lines I do not think it necessary to repeat, but their meaning is, in my opinion, so obvious, that I have no words to render it more so; I am, therefore, compelled to leave the reader to decide, whether I write sense, or Mr. Whipple cannot understand it.

"On the formula for the preparation of sulphate of potash," says Mr. W. "the writer of the paper is most fatally mistaken. In my opinion, the College have acted most judiciously in directing that the excess of acid be saturated with potash, instead of lime, for, in this instance, they employ a salt of a very inferior value to obtain one of a greater (and, by the bye, of some considerable importance to every manufacturing chemist), and, therefore, contrary to the opinion of the writer (of that paper), who says, "The College would have acted economically in imitating the directions of the Edinburgh Pharmacopœia, by saturating the excess of acid of the bisulphate, with lime instead of potash; by this the waste would have been avoided of using a salt of greater value to obtain one of less." A single importunity to any of the drug warehouses will convince him of his error." This case is very easily settled, and I shall make great allowances in Mr. W.'s favour, and yet the result will be in mine. Having made more than "a single importunity" to the requisite sources of information, I will admit that sulphate of potash is sold at a higher price than the subcarbonate; and this fact I may fairly claim as favourable to the accuracy of my statement; for the high price of the sulphate is the natural result of expensive means employed for its preparation.

The circumstances of the case are these: the College directs nitric acid to be procured by decomposing nitrate of potash with an equal weight of sulphuric acid; the residuum is consequently bisulphate of potash composed of 88 sulphate of potash and 40 dry sulphuric acid. The question, therefore, is, whether it is more economical to reject those 40 parts of dry sulphuric acid after saturation with lime, or to convert them into sulphate of potash by employing the subcarbonate.

Pearlash is impure subcarbonate of potash, but I will suppose it to be pure; it is sold at about 44 $\frac{1}{2}$  per ton; 40 parts of dry sulphuric acid require 70 of it for saturation; 22 of carbonic acid are expelled, and 88 of sulphate of potash produced. A short calculation will show that the cost of a ton of it thus prepared will be 35 $\frac{1}{2}$ .

Impure sulphate of potash is readily procurable in the market for about 15 $\frac{1}{2}$  per ton, and when the impurities and slight excess

of acid are removed by lime, I will admit that only three-fourths of it are obtained in the state of pure sulphate of potash, the cost of which will be 20*l.* per ton.

In making these statements, I suppose the trouble and expense of the operations to be equal.

The remarks next requiring observation are separated by a paragraph which I shall presently notice : they are, "Moreover I would ask, since economy be the maximum on which he has founded his examination, whether this salt could not be more economically obtained by employing potash in the process for forming the *ferrum præcipitatum*;" and "My remark relative to the *ferri subcarbonas*, will be seen in the note on sulphate of potash." It is to be observed that the College directs subcarbonate of soda for the decomposition of sulphate of iron in the preparation of what they term *ferri subcarbonas*, and Mr. W. calls *ferrum præcipitatum*. I have repeatedly endeavoured to obtain this compound by using subcarbonate of potash instead of soda, but from some unexplained cause, the carbonate of iron never contained so large a proportion of carbonic acid in the former case. I do not suppose that the carbonic acid has any immediate good effect, but when combined with protoxide of iron, it prevents its becoming peroxide, and consequently retains it in a more soluble state.

Mr. Whipple's next observations apply to what I have stated respecting the preparations of iron. "To attempt a definition of his remark on the preparations of iron, would be *Aquam arare*, wherefore I shall be obliged, if favoured with information, as to its abstract tendency. What must be the inference of an assertion like the following? 'That in the preparations of iron, there have been some alterations which are to be considered as amendments; but I am apprehensive that the good which has been done is more than counterbalanced by the omission of improvements, or the commission of errors.' Surely, if in the formulæ, that is, such as have been altered, amendments have taken place, how can we ascribe to the College a want of ability, or the commission of error?" I trust that most persons would understand that I conceive the College to have done some good, and more harm—the harm being of two kinds; *positive* by the commission of errors, and *negative* by the omission of improvements. I will give instances of each: the process for preparing *Ferrum tartarizatum* is improved; *Vinum ferri* is rendered worse by depriving a weak preparation of nearly one-third its strength. In the directions for preparing *ferrum ammuniatum*, about one-third only of the subcarbonate of iron ordered to be used are dissolved by the muriatic acid, and by the alterations introduced not only is waste incurred, but the apparent strength of the preparation is much greater than its real power. With respect to the omission of improvements, it is to be observed that more than one-fourth of the

sulphate of iron is wasted by continuing the directions for using too small a proportion of subcarbonate of soda; and a larger quantity of solution of subcarbonate of potash should have been directed in preparing the *liquor ferri alcalini*.

With respect to my proposal for substituting strong acetic acid diluted with water for distilled vinegar, Mr. Whipple says, "The acidum aceticum fortius diluted with water does not answer for the purpose of making the liquor plumbi subacetatis. I have frequently tried it, and ever been unsuccessful, for as soon as it assumes the density, as required in the Pharmacopœia, it becomes opaque, which cannot be removed by filtration." I have no doubt of the accuracy of Mr. Whipple's statement when he admits that he has "ever been unsuccessful;" and he will continue to be so while he employs impure acetic acid; this must have been the case, for I assert that the *acidum aceticum fortius* diluted with water, *does* answer for the purpose of making the liquor plumbi subacetatis, a perfectly clear and colourless solution being immediately obtainable by filtration.

## ARTICLE XI.

### ANALYSES OF BOOKS.

*An Explanatory Dictionary of the Apparatus and Instruments employed in the various Operations of Philosophical and Experimental Chemistry. With 17 Quarto Plates. By a Practical Chemist. London. Boys. pp. 295. 16s.*

SEVENTEEN well executed quarto plates (for such they really are) for sixteen shillings can hardly be a bad bargain, and if the text at all equal the engravings, in matter as well as type, it must be a very cheap one, at least as books go now. At all events, Mr. Adlard, the engraver, and Mr. Green, the printer, have done *their* duty, and the paper does not disgrace the stationer who sold it. So much for the getting up; and we assure our readers, it is no small part of the art and mystery of book-making in these days of bibliomaniacal fastidiousness. We could wish indeed, for our own sakes, that matters would take a turn, and the price of books descend a little more to the level of our "cold" purses; but whilst our friend Mr. Dibdin continues to treat us with such luxuries as his *Strasbourg Cathedral*, *Année of Brittany*, and the *View of Rouen on the Road to Havre*, &c.\* we cannot help wishing him to persevere in his splendid course, though he half ruins us with the irresistible temptations. But to the work before us.

\* See his *Tour*, the most beautifully illustrated work of the kind of the present day.

We learn from the Preface that the design of the present publication is to supply the want of plates in most of the elementary treatises on chemistry, and to assist students who attend the public chemical lectures, but have not sufficient time or opportunity to examine the furniture of the lecture-table, so as to be enabled perfectly to comprehend the construction and principles of every article of apparatus. The author acknowledges that he has made free use of the best authorities, and at the same time lays claim to many original remarks and explanations.

The first chapter is devoted to a dissertation on the general nature of chemical *apparatus* and *instruments*; and though the remarks are somewhat diffuse, the young chemist will find in them, on the whole, a good deal of useful matter; but we think the necessity of having shelves, drawers, cupboards, bottle brushes, sponges, towels, &c. &c. in the laboratory, might have been left to the tyro's own sagacity to find out, who will probably not be long before he discovers that chemical operations are marvellously apt to make dirty hands, and that he must be no niggard of his trouble in often cleaning his flasks, precipitating glasses, retorts, &c. &c. as well as his own fingers, before he quits the fumes of the laboratory for the perfumes of the drawing room.

This chapter also contains a pretty long list of instruments and utensils, and another of tests, &c.; in the latter of which, the same articles are in several instances repeated under different heads; for instance, under the head Tests, we find carbonate of ammonia, nitrate of barytes, nitrate of lead, sulphate of iron, &c. and the same substances occur again in the next page under the head Salts. Is this for the sake of amplification, or from inattention? It is bad at all events. Next come heat and fuel, amongst which the author has omitted to notice the new substance obtained from the distillation of wood, and, not very properly perhaps, called naphtha; it is a cheap and excellent substitute for spirits of wine. This chapter concludes with some remarks on the method of conducting experiments, including some good advice to young operators, extracted from Macquer and Dr. Henry.

The very small space we can afford to our further remarks on this volume will allow of only a few short extracts from some of the articles, as a specimen of the general style of the work.

*Blowpipe*.—Figures and descriptions are given of Bergman's, Black's, and Wollaston's, which may be considered as *legitimate* blowpipes, as they are supported by the hand, and the blast urged by the breath of the operator, the only possible method of giving all the nice varieties of flame and position that are required in the dexterous management of this admirable little instrument. There are also figures of Brooke's Oxy-hydrogen Blowpipe, an useful instrument, when we wish to throw an intense heat on a

single point; of a self-acting blowpipe, the old æolipile, not worth one farthing; of a blowpipe with a self-adjusting candlestick (a self-adjusting fiddlestick to Mr. François Cramer's violin would be about as necessary or useful); and a blowpipe, with a stop-cock, to be used with a bladder.

The article annexed to *blowpipe* (Bergman's) is made up of the substance of the observations found in the best works on the subject, and is culled from Bergman's Treatise, *De Tubo Ferruminatorio*, Berzelius's *Essai de l'emploi du Chalumeau*, &c. Our author tells us, that "The best *kind of flame* for blowing through with the blowpipe, is a thick wax or tallow candle." This is new to us; we did not before know that a candle and its flame are the same thing; but however that may be, we think the recommendation erroneous, and prefer a low lamp, supplied with oil, to any candle whether of wax or tallow; for the lamp wants no snuffing; the wick, when once well trimmed, will last a long time without requiring the least alteration, and we avoid the abominable nuisance of having our hands or instruments smeared with melted wax, or "stinking tallow." The advantage also of being able to adapt the size of the wick to the nature of the operation, is materially in favour of the use of the lamp in preference to a candle.

"In using the blowpipe, the following observations should be attended to. The end of the nozzle pipe must be just entered into the flame, and the current of air will then throw out a cone or dart of flame from the opposite side. If it is well managed, this dart or cone will be very distinct and well defined. Care must be taken that the stream of air does not strike against any part of the wick, as it would then be disturbed and split into several parts. The jet or blast of air must be delivered somewhat above the wick; and as unless the flame was considerable there will not be sufficient for the stream of air to act upon, for this reason the wick is best to be opened, because it then exposes the largest surface, and produces the greatest flame; the stream of air from the pipe should then be directed through the channel or opening between the wick, so as to produce a cone the most perfect and brilliant, directed downwards at an angle of about 45 degrees."

These directions are not amiss, but require some qualifications. The position of the nozzle of the blowpipe with respect to the flame must depend on the effect required; if an oxidating flame be wanted, the extremity of the blowpipe should be inserted to some distance in the flame; for a reducing flame, it must be drawn further back, and as to the form of the wick, except in cases where a large flame is required, it is best to let it be cylindrical and unbroken. The flame of a wick of this form will be found the most convenient and manageable in all delicate experiments.

The author has given no figure of Gahn's blowpipe, which we find superior to any other. Dr. Wollaston's, like all his inventions, is perfect for the object proposed, viz. portability; but for constant use, one with a reservoir is preferable, and of those we most approve of the form given by Gahn.

The article Hygrometer is one of the longest and best in the book; and we give the author credit for having dwelt amply on the admirable instrument invented by Mr. Daniell. A woodcut, not very neatly executed, is given of this hygrometer, and of Leslie's, as well as of some other apparatus.

Under the head Hydrometer, Nicholson's useful table of the correspondence of the degrees of Beaumé's hydrometers for salts and spirits, with their actual specific gravities at 55° Fahr. is given, as well as Gilpin's valuable tables of the specific gravities of alcohol of different strengths, and at different temperatures. Under the article Measure Glass, also, several useful tables are introduced, and generally through the work much information, which both the student and proficient may refer to with advantage.

We are surprised that the author has not noticed the pyrometer invented by Mr. Daniell; a long account is given of Wedgwood's, which, it is now known, is an instrument of very little service, since the clay pieces which serve to indicate the temperature, contract as much by a lower degree of heat long continued, as they do by the most intense. We have also looked in vain for a figure of Mr. Cooper's excellent apparatus for the analysis of organic substances. Neither that nor Dr. Prout's are noticed. *Hiatus valdè deplendus!* However, on the whole, we think the Explanatory Dictionary cannot fail to be useful to a large class of chemical readers, and hoping the author will fill up the desiderata in the next edition, we wish he may soon have the opportunity of doing so, and bid him heartily farewell.

## ARTICLE XII.

### *Proceedings of Philosophical Societies.*

#### ROYAL SOCIETY.

THE meetings of this learned body, as we have already mentioned, were resumed on the 18th of November, 1824; when Douglas C. Clavering, Esq. Capt. R.N. was admitted a Fellow of the Society, and the following communications were read:—

The Croonian Lecture, by Sir E. Home, VPRS. :—In pursuing his researches in minute anatomy, the author stated, at the commencement of this Lecture, he had again availed himself of the skill and accuracy of Mr. Bauer; and in this respect he

remarked, he enjoyed an advantage which no anatomist had ever before possessed, and which, perhaps, might never again occur to any one.

Proceeding to the immediate subject of the Lecture, Sir E. Home stated, that Mr. Bauer had discovered *nerves* both on the foetal and the maternal surface of the placenta: they extend over the arteries in a kind of trellis-work, and each fibre, when highly magnified, seems to consist of globules connected together: they are altogether distinct from any sort of arterial or venous tubes, and reflect the light like white human hairs.—The arrangement of the nerves on the placenta of the seal and fallow-deer was then described.—Sir T. S. Raffles, whose loss of the most valuable collection of subjects of Natural History ever formed in the East Indies, the author observed, every one must feel for, presented him with the pregnant uterus of the Sumatran tapir, in which there is no placenta, the umbilical cord passing from the foetus directly to the chorion; and in this case the nerves were found in the flocculent part of the latter organ.

Sir Everard next gave an account of the distribution of the nerves belonging to the organs of generation in the human female, and in those of the quadruped and bird.—He had long since suspected that wherever there were blood-vessels there were nerves, and that the latter, besides their office of conveying sensation, were concerned in the formation of arteries; and from the extreme vascularity of the placenta, he had inferred their existence in that organ. Mr. Bauer's verification of this inference threw great light upon various facts, hitherto unexplained, depending upon the connexion of the mother with the foetus;—it showed that the brain of the mother is connected with all its nerves. Thus it explained the circumstances, of a foetus formed without brain; of children dying on the too speedy division of the navel-string; and of the various effects ascribed to the influence of the imagination of the mother on the offspring, of which there were too many authenticated instances to reject, though from their not having taken place in certain particular cases, they had been considered as accidental. The Lecture closed with an account of some instances of this kind which had come within the immediate knowledge of the author. One of them was that recorded in the Philosophical Transactions, of the mare, which, having first had a foal by a quagga, had afterwards three foals successively by a Persian horse, all of which were marked like the progeny of the quagga. Illustrative drawings by Mr. Bauer were annexed to this Lecture.

On the Changes undergone by the Ovum of the Frog, during the Production of the Tadpole. By the same Author.

Sir Everard Home having investigated the gradual changes produced by incubation in the ova of warm-blooded animals, by examining the formation of the chick, had now extended his

researches, with Mr. Bauer's aid as before, to the cold-blooded class of animals. The general successive steps of the process had been ascertained to be the same in both classes. Mr. Bauer's drawings of those which took place in the ovum of the frog were annexed to the paper.

Nov. 25.—At this meeting Richard Penn, Esq. was admitted a Fellow; and the name of William Scoresby, Jun. Esq. ordered to be inserted in the printed lists of the Society: the following paper was read:—

A New Method of calculating the Angles under which the Planes of Crystals meet; by W. Whewell, MA. FRS. and Fellow of Trinity College, Cambridge.

In this paper, of which the introduction only was read, the details being purely mathematical, the author proposed to substitute for the mode of calculating the angles of crystals hitherto employed, in which different methods are used, according to the relation of the different crystals to their nuclei, a few simple formulæ of universal application; and also to substitute for the arbitrary and inelegant notation by which the planes of crystals have heretofore been designated, a simple and expressive notation of corresponding symbols.

Nov. 30.—This being St. Andrew's Day, the anniversary meeting of the Royal Society was held at the Society's apartments in Somerset House.

The President, Sir Humphry Davy, took the Chair at twelve o'clock, and delivered an eloquent address to a large number of the members assembled on the occasion. We are happy to be able to lay before our readers a faithful and pretty copious abstract of that able and impressive composition.

After reading the list of members whom the Society has lost by death in the course of the preceding year, in which the names of Lord Byron, Mr. Lowry, and Baron Maseres occurred, Sir Humphry Davy observed, that the only character which he was called upon to notice, as a contributor to the *Philosophical Transactions*, was that of Baron Maseres, whom he described as having belonged to the old mathematical school of Britain, and who, through a long life, devoted much of his leisure, and a portion of his fortune, to the pursuit and encouragement of the higher departments of algebra and geometry. His love of science was of the most disinterested kind, as is shown by the nature of his publications, and his liberality in encouraging the publications of others. He died in extreme old age, having almost outlived his faculties.

The President then announced that the Council had awarded the medal of Sir Godfrey Copley's donation, for the present year, to the Rev. John Brinkley, D. D. Andrew's Professor of Astronomy in the University of Dublin, and President of the Royal Irish Academy.

To some of the members of the Society, who have not followed closely the usages of the Council, a question may arise, why, in two successive years,\* the cultivators of a science, which, during that time, has been distinguished by no remarkable discoveries, should receive the highest honours which this philosophical association has to bestow!

The progress of science has no annual periods; and when a medal is to be bestowed every year, not merely important scientific facts, but likewise trains of useful labours and researches must be considered, and the zeal, activity, and knowledge of those persons, who, having been contributors to the Transactions, must be considered as competitors, are to be taken into the account.

It has now and then happened that the Royal Society has had the felicity to mark some great and brilliant discovery, such as that of the aberration of light, or the magnetic effects of electricity, by this token of its respect; but in general the medal is, of necessity, bestowed for contributions of a more humble character; to reward those laborious philosophers who have enlightened science by correct observations or experiments; or those sagacious inquirers, who, by accurate reasonings, or ingenious views, lay the foundations for new researches, or new theoretical arrangements, or applications of science to the uses of life. If any one department of natural knowledge requires encouragement more than another, it is Astronomy; for having arrived at a mature state, and presenting few striking objects of discovery, it can only be perfected by the most minute, laborious, and delicate inquiries, which demand great attention, great sacrifice of time, and often of health, since they must frequently be carried on at a period usually devoted to repose.

Dr. Brinkley has long been known as an enlightened and profound mathematician, and his papers in the Memoirs of the Royal Irish Academy, and some of those in the Philosophical Transactions, contain abundant proofs of his skill in the higher departments of analysis. Whoever, said the President, is in possession of the higher resources of the mathematical sciences, may be considered as gifted with a species of power applicable to every department of physical knowledge. It is, indeed," for this species of knowledge what muscular strength is for the different branches of human labour. It not only generalizes the results of experiment and observation, but likewise corrects them, and leads to new and more refined methods of investigation. The guide of the mechanical and pneumatical philosopher, and the useful assistant of the chemist, it is of still more importance to the astronomer, whose results depend entirely upon magnitude, time, and motion.

\* The Copley medal was last year given to the Astronomer Royal.—*Ed.*

Endowed in so high a degree with *one* of the essential characters of an accomplished astronomer, his various and later communications to the Royal Society show that Dr. Brinkley is equally distinguished as a laborious, acute and accurate observer.

After stating the several subjects of Dr. Brinkley's seven communications to the Royal Society, published in the Philosophical Transactions, and justly eulogizing their extraordinary merit, Sir Humphry Davy proceeded to notice the two great leading questions of astronomy, concerning which the Astronomer Royal and Dr. Brinkley are at issue; namely, 1. The sensible parallax of some of the fixed stars; and, 2. The apparent southern motion or declination of parts of the sidereal system.

It is well known that sensible parallax is denied by Mr. Pond, and believed to exist by Dr. Brinkley; whilst, on the contrary, southern declination is denied by Dr. Brinkley, and believed to exist by Mr. Pond. I mentioned, the President continued, in announcing the award of the medal last year, that the Council of the Royal Society had no intention of giving its sanction to the opinions of the Astronomer Royal, or of attempting to decide on these important and difficult questions. I again feel it my duty to make the same observation on this occasion, and to state that the *general* labours of Dr. Brinkley in the most difficult parts of astronomy, and the approximation that he has made to the solution of a great problem, and the high merits of his philosophical inquiries, are the sole grounds on which the Copernican medal has been bestowed.

When Copernicus first developed that sublime system of the planetary worlds, which has since been called after his name, he was obliged to suppose the fixed stars at an almost infinite distance, and the astronomical instruments of that day offered no means even of attempting the discovery of their parallax.

The importance of such a discovery was, however, immediately felt; as a demonstration of it would in fact become likewise an absolute demonstration of the Copernican system of the universe.

Gallilæo seems to have suggested the method of inquiring for parallax, by examining the relative position of double stars, one of large, and the other of small magnitude, at the two extremities of the earth's orbit; a method founded on the supposition that the stars do not greatly differ in absolute size. This method, which was likewise strongly recommended by Dr. Wallis, was first, I believe, said the President, practised, and pursued with great sagacity and industry, and with instruments of extraordinary magnitude and perfection, by the late Sir William Herschell, and, in following his path, by Mr. Herschell and Mr. South. Though it has afforded many important results with respect to the proper motions of the stars and the arrangement and groups

of those heavenly bodies, it has as yet furnished no observations forming data for reasoning on the distances of the fixed stars from the sun.

The other method, and that which has been most insisted upon, seems likewise to have originated with the illustrious Florentine philosopher, namely, that of observing stars, about the summer and winter solstice, in or near the zenith, for the purpose of avoiding the errors of refraction, by fixed instruments. The celebrated Robert Hooke, who erected, at Chelsea, a telescope 36 feet long for examining  $\gamma$  Draconis, imagined that he had discovered a very considerable parallax for this star; but Hooke's observations were contradicted by Molyneux.

Flamsteed drew a similar conclusion from his experiments on the pole star, but the results which he attributed to parallax were explained by Bradley's great discoveries of the aberration of light, and the nutation of the earth's axis; and it is remarkable that Hooke reasoned correctly on inaccurate observations, whilst Flamsteed formed wrong conclusions from exceedingly correct results.

James Cassini, in observing Sirius, attributed a parallax of  $6''$  to this star; and La Caille, from observations made at the Cape of Good Hope, supposed it to be  $4''$ .

Piazzi, whose conclusions are given with great diffidence, in researches pursued from 1800 to 1806, supposes that several of the fixed stars exhibit parallax. He assumes for Sirius nearly the same parallax as La Caille, for Procyon  $3''$ , and for Capra less than  $1''$ . In all these observations, nothing like southern motion, it must be confessed, had ever been suspected. Dr. Brinkley, in 1810, rated the parallax for  $\alpha$  Lyrae at  $2\frac{1}{2}''$ . The general conclusions of the Astronomer Royal from observations made both with a fixed instrument, and with the mural circle, are unfavourable to the existence of sensible parallax for any of the fixed stars, and he refers apparent parallax to imperfections in the instruments used in the observations, and offers as a proof, the *diminution* of the indications in proportion as instruments have become more delicate; and estimating the Greenwich, as superior to the Dublin Circle, thus accounts for the difference of his results and those of Dr. Brinkley.

Dr. Brinkley, in reply, does not allow the superiority of the principle of the Greenwich instrument, and shows the consistency of the Dublin instrument with itself, by numerous observations which place its permanent state beyond all doubt. The results of 62 observations on  $\alpha$  Lyrae, in 1811, give the mean difference between the summer and winter zenith distances as  $1''.32$ ; and repeated observations, in the last ten years, give sensible parallax, though with less consistency, for  $\alpha$  Aquilæ,  $\alpha$  Cygni, and Arcturus; but none for  $\gamma$  Draconis. Dr. Brinkley seems entirely convinced of the accuracy of his general conclu-

sions. If any circumstances depending upon change of temperature, flexure of the instrument, or other causes of error existed, why, he says, should they not be *general for all the stars*? Why should such causes exist for  $\alpha$  Lyræ, and not for the pole star, which shows no sensible parallax?

On the question of southern motion, Dr. Brinkley compares Mr. Bessel's, Mr. Pond's, Mr. Piazz's, and Dr. Bradley's Catalogues, and after endeavouring to prove discordance in the Astronomer Royal's mode of applying the data in these Catalogues to the question, he says, "from the weight of external testimony adduced, it will, I think, be readily conceded to me, that the southern motion does not exist, and that it must be regarded as an error, belonging to one or both of the Greenwich Catalogues of 1813 and 1823."

Such is the state of these two questions; they are not, however, questions of useless controversy, nor connected with hostile feelings: the two rival astronomers seem equally animated by the love of truth and of justice, and have carried on their discussions in that conciliating, amiable, and dignified manner, which distinguishes the true philosopher. I cannot give a stronger proof of this, than in stating that the Astronomer Royal was amongst the first of the members of the Council to second and applaud the proposition for the award of this day.

After some further observations on the subject of parallax, the President remarked, that it is to be regretted that no star has yet been observed absolutely in the zenith, which might easily be done, and in a part of the globe, for instance under the equator, where almost precisely the same circumstances of temperature, moisture, and pressure of the atmosphere, would exist in summer and winter. An instrument fixed on granite, or an aperture made in a solid stratum of rock, would destroy the possibility of interference from foreign causes, and reduce the problem to the simplest possible conditions.

Sir Humphry Davy then congratulated the Society on the great progress that is making in scientific inquiry, and the means for procuring the necessary instruments, and paid a well merited tribute of respect to several of the most eminent astronomers of the present day, and to those artists, especially Troughton, Dolland, Reichenbach, and Fraunhofer, whose genius and industry have brought philosophical apparatus to its present high state of perfection.

The President then concluded this brilliant address nearly in the following words:—

There is no more gratifying subject for contemplation than the present state and future prospects of astronomy; and when it is recollected what this science was two centuries ago, the contrast affords a sublime proof of the powers and resources of the human mind.

*New Series, VOL. IX.*

The notions of Ptolemy concerning Cycles, and Epicycles and the moving spheres of the heavens, were then current; the observatories were devoted rather to the purposes of judicial astrology than to the philosophy of the heavenly bodies; to objects of superstition rather than of science.

If it were necessary to fix upon the strongest characteristic of the superiority of modern over ancient times, I know not whether the changes in the art of war from the application of gunpowder, or in literary resources from the press, or even that wonderful power created by the steam-engine, could be chosen with so much propriety as the improved state of astronomy.

Even the Athenians, the most enlightened people of antiquity, condemned a philosopher to death for denying the divinity of the Sun; and as to the other great nations, cotemporary with the Athenians, it will be sufficient merely to mention their idolatry, or utter ignorance with regard to the laws or motions of the heavenly bodies.

Take the most transient and the simplest view of the science as it now exists, and what a noble subject for exultation! Not only the masses and distances of the sun, the planets and their satellites, are now known, but even the weights of bodies upon their surfaces ascertained, and all their motions, appearances, and changes, predicted with the utmost certainty for years to come, and even carried back through past ages, to correct the chronology, and fix the epochas in the history of ancient nations. Attempts have even been made to measure the almost inconceivable distances of the stars, and with this, what sublime practical and moral results! The pathless ocean navigated, and in *unknown* seas, the exact point of distance from known lands ascertained. All vague and superstitious notions banished from the mind, which, trusting to its own powers and analogies, sees an immutable and eternal order in the whole of the universe, intended after the designs of the most perfect beneficence, to promote the happiness of millions of living beings, and where the whole of created nature offers its testimony of the existence of a Divine and Supreme Intelligence!

The President then delivered the medal to Mr. Baily, to be transmitted to Dr. Brinkley, begging him to assure that gentleman of the respect and admiration of the Royal Society, who receive his communications, presiding, as he does, over another kindred scientific body, not merely with pleasure, but with gratitude, and who trust that he will continue them both for the advancement of astronomy, and for the increase of his own high reputation!

The Society then proceeded to choose their Council and Officers for the ensuing year; and the following were declared duly elected:

*Of the Old Council.*—Sir Humphry Davy, Bart.; W. T.

Brande, Esq.; S. Goodenough, Lord Bishop of Carlisle; Major T. Colby; J. W. Croker, Esq.; D. Gilbert, Esq.; C. Hatchett, Esq.; Sir E. Home, Bart.; J. Pond, Esq.; W. H. Wollaston, MD.; T. Young, MD.

*Of the New Council.*—W. Babington, MD.; F. Baily, Esq.; J. G. Children, Esq.; J. W. Viscount Dudley and Ward; J. F. W. Herschel, Esq.; Capt. H. Kater; T. A. Knight, Esq.; A. Mac Leay, Esq.; Sir T. S. Raffles, Knt.; Edward Adolphus, Duke of Somerset.

*President.*—Sir H. Davy, Bart.

*Treasurer.*—D. Gilbert, Esq.

*Secretaries.*—W. T. Brande, Esq.; and J. F. W. Herschel, Esq.

*Dec. 9.*—Charles Mackintosh, Esq. was admitted a Fellow; M. Thenard was elected a Foreign Member; and the following communications were received.

Three extensive series of Astronomical Observations made at the Observatory of Paramatta, New South Wales; communicated by Sir Thomas Brisbane.

Explanation of an Optical Deception in the Appearance of the Spokes of a Wheel seen through Vertical Bars. By P. M. Roget, MD. FRS.

A portion only of this paper was read.

*Dec. 16.*—The name of Dr. John Thomson, of Edinburgh, was ordered to be inserted in the printed lists of the Society; and the reading of Dr. Roget's paper was concluded.

The spokes of a revolving wheel appear curved when viewed through the intervals of a series of vertical bars, such as those of a palisade, or a Venetian window-blind. The spokes on each side of the upper one, which has arrived at the vertical position, appear bent upwards; and the curvature of each spoke increases accordingly as it is more distant from the uppermost one. The direction of the curvature is the same, whether the wheel be moving to the right or to the left of the spectator. The appearance takes place only when the wheel is revolving with a certain velocity, and remains the same whatever greater velocity is given to the wheel, as long as the spokes continue visible. The author states the results of experiments illustrating the influence of various circumstances on these illusive appearances; and infers from them that the combination of a progressive with a rotatory motion is essential to their production. He explains them on the well-known physiological principle of the continuance for a certain time of an impression made on the retina; and shows that not only all the ordinary phenomena accord with his theory, but that, by means of it, the result of more complicated combinations may be anticipated. The paper concludes with a mathematical investigation of the curves thus generated; of which the general equation and leading properties are given.

The following communication was also read:—

On a new Photometer; by A. Ritchie, AM. \* communicated by the President.

The principles on which the indications of this instrument depend, are, that radiant heat does not pass through thick plates of glass, but is conducted through them in the same manner as through opaque bodies; that light expands in the same manner as heat the substances which absorb it; and that the intensity of light varies inversely as the square of the distance. Mr. Ritchie's photometer, however, differs essentially from that of Prof. Leslie. Its delicacy is such that it is very sensibly affected by the light of a candle at the distance of 20 or 30 feet, while no effect is produced on it by a hot ball of iron radiating a much greater quantity of heat. When exposed to several lights at different distances, it expresses their intensity according to the law just stated.

### ARTICLE XIII.

#### SCIENTIFIC NOTICES.

##### CHEMISTRY.

##### 1. *Saline Efflorescence upon the Surface of Bricks.*

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

*Lisson Grove.*

WALKING some time since with a friend in the neighbourhood of St. John's Wood Road, where a considerable number of cottages in the Italian style, surrounded by gardens and inclosed with brick walls, have lately been erected, our attention was attracted by observing the irregular distribution of a white coloured substance upon the surfaces of some of these garden walls. Upon closer inspection, a pretty thick and extensive efflorescence became apparent, a portion of which when scraped off and applied to the tongue communicated a strong and disagreeable saline taste. A quantity of this saline substance was collected, and on being submitted to a slight chemical examination, it soon became evident that it was almost entirely composed of sulphate of soda, blended with minute portions of muriate of lime and magnesia.

I am aware that sulphate of soda has occasionally been met with as an efflorescence upon old walls on the Continent and elsewhere, but I do not think that it is a very common occurrence in this country; and may it not be asked, whether the solidity of walls built of such materials is not likely to be materially diminished?

The salt evidently, in the first instance, crystallizes upon the

surfaces of the brick, and in a dry state of the atmosphere, effloresces. The action of rain speedily dissolves the efflorescence thus formed, and another portion of salt will be quickly determined to the surface; should this succession of changes continue to go on, the bricks may soon be expected to become porous, and their consequent disintegration must happen in a short time.

If you consider this notice worth inserting in the *Annals of Philosophy*, it may probably be the means of inducing those whom it may more immediately concern, to attend to a circumstance of which, perhaps, they are not aware.

I remain, Gentlemen, your very obedient servant,

M. W.

## 2. Solubility of Oxide of Cobalt in Ammonia.—Cobaltic Acid.

Oxide of cobalt does not appear capable of dissolving directly in ammonia, and a combination between the two substances can take place only under the two following circumstances:— 1. Either the oxide of cobalt combines with an acid, and in this state forms a double salt with the ammonia, which is also combined with the same acid; as, for example, in the carbonate of oxide of cobalt and ammonia, nitrate of oxide of cobalt and ammonia, &c. 2. Or, when the proportion of acid is insufficient to saturate both the oxide of cobalt and the ammonia, as, for example, when a neutral salt of cobalt is treated with an excess of ammonia, there is formed a small quantity of the double salt, and the greater proportion of the oxide precipitates in blue coloured floccs, which, so long as oxygen gas is excluded, do not redissolve. If oxygen gas be admitted, it is rapidly absorbed; the blue floccs at the same time assume a green colour, and gradually disappear, yielding a brown coloured solution. If a salt of cobalt containing an excess of acid be employed, or if there be previously added a sufficient quantity of the corresponding ammoniacal salt, the addition of an excess of ammonia occasions no precipitate, and there is obtained a pale red coloured liquid; in the case of nitrate of cobalt, this liquid undoubtedly contains nitrate of cobalt and ammonia, and deposits red coloured crystals. This solution also is capable of absorbing oxygen gas, and its colour is thereby changed to a brown. The maximum amount of absorption is in the proportion nearly of one equivalent of oxygen to one equivalent of oxide of cobalt; consequently the cobaltic acid thus formed contains half an equivalent more of oxygen than the hyperoxide of cobalt.

If the above ammoniacal liquid, previously saturated with oxygen, be committed to a rapid spontaneous evaporation, it yields a compound of *ammonia with nitric and cobaltic acids*, in brown coloured, apparently four-sided prisms with square

bases. This salt dissolves without undergoing decomposition in dilute liquid ammonia, forming with it a brown coloured solution: in water it dissolves only partially, azote being at the same time disengaged, and hyperoxide of cobalt precipitated. Exposed to the air, it is rapidly decomposed, and becomes dull and red coloured: it seems probable that the decomposition is principally occasioned by the absorption of carbonic acid.—*Leopold Gmelin.*

#### MINERALOGY.

##### 3. *Composition of Garnet.*

When writing the short notice on garnet contained in our number for Nov. last (vol. viii. p. 388), we were not aware that a systematic examination of this mineral had been already undertaken and accomplished. Having since received the entire volume of the Swedish Transactions for 1823, we find in it a memoir by Wachtmeister, containing a description and analysis of 13 varieties of Garnet, all from different localities and geological positions. With only one exception they all proved to be constituted in conformity with the formula which we gave in our notice, namely, an atom of a silicate of a base containing three atoms of oxygen (as alumina, peroxide of iron), combined with an atom of a silica of a base containing two atoms of oxygen (as lime, magnesia, protoxide of iron, protoxide of manganese). Whenever a genus becomes so diversified as is the case with garnet, it is of the utmost consequence, in a mineralogical point of view, to investigate the connexion which subsists between the chemical composition of each variety, and its external and physical characters, such as its specific gravity, hardness, colour, transparency. We have, therefore, arranged the most important of Wachtmeister's results in the form of a synoptical table; by means of which the mutual relations between the principal characters of each variety will be made at once apparent to the reader. In a geological point of view, it is of no less importance to trace the degree of similarity which subsists between the composition of a simple mineral and its matrix; and as the garnet appears to surpass almost every other class of minerals in the remarkable extent to which its composition is influenced by that of the substance in which it exists imbedded, or upon which it rests, we have allotted a column to the matrix of each variety, or, where that has not been mentioned, to the minerals with which it is found associated.

Locality.	Matrix.	Form.	Fracture.	Colour.	Transparency.	Hr.-hess.	Sp. Gr. Mineral. Form.
1. Engsö . . . . .	Felspar.	Trapezoidal.	Large f. foliated; small f. uneven.	Dark red.	Transparent on the edges.	Scratches quartz.	4.236 f } S + A S. mn
2. New York . . . .	Mica slate.	Regular rhomb. do- decahedron.	Foliated.	Dark violet red.	Ditto.		3.90 f } S + A S. mn
3. Hallandsås . . .			Even passing into conchoidal and uneven.	Light red.	Transparent on the edges.	Ditto.	4.188 f } S + A S. mn
4. Halland . . . . .	A rock composed of hornblende and quartz.	Massive.	Slaty, or large foliated.	Very light red.	Ditto.		4.043 } Ditto. mn
5. Vesuvius . . . . .	With sodalite.	Elongated rhomb. do- decahedron.		Light reddish brown.	Transparent in thin fragments.	Scratches glass, but not quartz.	3.498 f } S + A } S. mn
6. Langenshyttan	Reddish.	Massive.	Even.	Yellow and brown- ish yellow.	Transparent on the edges.		3.965 C } S + F S. mn
7. Altenau . . . . .	Magnetic iron ore.	Elongated rhomb. do- decahedron.	Uneven, inclining to granular.	Dark yellow.	Slightly scratches quartz.		3.871 Ditto.
8. Hesselkulla . . .	Calcareous spar . .	Massive.	Even.	Dark grey, inclining to green.	Transparent on the edges.	Scratches glass, but not quartz.	C } S + A } S. mn
9. Ditto . . . . .	Ditto.	Ditto.	Ditto.	Dark grey, inclining to brown.	Opaque.	Scratches glass, but not quartz.	C } S + A } S. mn
10. Arendal . . . . .	Ditto.	Elongated rhomb. do- decahedron.	Ditto.	Coal black.	Ditto.	Not scratched by quartz.	3.157 M } S + A S. mn
11. Ditto . . . . .		Trapezoidal.	Granular to fine foliated.	Dark brownish black.	Ditto.	Slightly scratches quartz.	3.665 C } S + A } S. mn
12. Kamschacka (River Wilhi) . . .		Ditto.	Even, inclining to conchoidal.	Pale green.	Transparent.	Scratches quartz.	3.64 C S + A S.
13. Klemetsaune, in Norway . . . . .	Albite ?	Rhomb. do- decadron.		Reddish brown.	Transparent in thin fragments.		3.851 f } S. mn AS + C } S.

The numerical results of his analyses were as follows :

	1	2	3	4	5	6	7
Silica .....	40.60	42.51	41.00	42.000	39.93	35.10	35.64
Alumina .....	19.95	19.15	20.10	21.006	13.45	—	—
Oxide of iron .....	—	—	—	—	14.90	29.10	30.00
Lime .....	—	1.07	1.50	4.980	31.66	26.91	29.21
Magnesia .....	—	—	6.04	4.320	—	—	—
Oxidule of iron .....	33.93	33.57	28.81	25.180	—	—	—
Oxidule of manganese	6.69	5.49	2.88	2.375	1.40	7.08	3.02
Potash .....	—	—	—	—	—	0.98	2.35
Loss .....	—	—	—	0.145	—	0.83*	—
	101.17	101.79	100.33	100.000	101.34	100.00	100.22

	8	9	10	11	12	13
Silica .....	38.125	37.993	42.450	40.20	40.55	52.107
Alumina .....	7.325	2.712	22.475	6.95	20.10	18.035
Oxide of iron .....	19.420	28.525	—	20.50	5.00	—
Lime .....	31.647	30.740	6.525	29.48	34.86	5.775
Magnesia .....	—	—	13.430	—	—	—
Oxidule of iron .....	—	—	9.292	—	—	23.540
Oxidule of manganese	3.300	0.615	6.273	4.00	0.48	1.745
Potash .....	—	—	—	—	—	—
Loss .....	0.183*	—	—	—	—	—
	100.000	100.585	100.415	101.13	100.99	101.202

The garnet which, in its composition, constituted an exception to all the other varieties, is the one whose leading characters are given in No. 13. It contains so large a proportion of silica, that one-half of the bases must be supposed to exist in the state of bisilicates. Wachtmeister, however, is of opinion, that this variety may for the present be left out of view, because it is even to the naked eye distinctly intermixed with a foreign mineral, and it is not to be supposed that the composition of a substance evidently impure, can be admitted to invalidate a general law, which all the twelve other varieties coincide in indicating. The garnet, indeed, possesses a peculiar tendency to crystallize indiscriminately with other mineral compounds, from which it differs essentially in chemical constitution; and the presence of such foreign bodies, so far from being an obstacle, appears even to favour its crystallization. These foreign admixtures may be often recognized in the form of dull spots on the surface of the most regularly formed crystals.

#### 4. Collection of Minerals.

In proof that mineralogy is not on the decline on this side of the water, we may allude to the purchase of Haüy's collection not long ago by his Grace the Duke of Buckingham; and we

\* Including some carbonic acid.

have now to acquaint our readers with Mr. Heuland's purchase of that of the Marquis de Drée. The Parisians, however, have still to admire the private collection of the late King of France, under the superintendence of Count Bournon, that of the Jardin des Plantes, and another, highly instructive, at the College of Mines. Besides every Professor of Mineralogy at Paris has a collection, and that of Monsieur Brochant de Villiers is now the most valuable of any private individual there. With the exception of two other private collections of *peculiar note* on the Continent, all the others from thence have by degrees found their way to this country.

#### 5. *Supports for Minerals before the Blowpipe.*

Our readers will remember the interesting paper by Mr. Smithson on this subject, which appeared in the *Annals of Philosophy*, vol. vi. p. 412, N. S. Lieut.-Col. Totten, of the United States, has lately published some experiments on the same subject. His process is a modification of that adopted by Mr. Smithson. We extract a part of his memoir.

"Not being able to obtain any clay sufficiently refractory for my purpose, though I tried the German and the English (Stourbridge) clay, used for crucibles by glass-blowers, and two or three specimens called pipe-clay, I had recourse to the minerals which I designed to expose to the action of the flame; this is Mr. Smithson's third process. Instead, however, of taking upon the point of the wire a very minute portion of the paste made of the powdered mineral, according to Mr. Smithson's method, I formed a paste by mixing the powder with very thick gum water, and rolling a little of it under the finger, formed a very acute cone, sometimes nearly an inch in length, and generally about a twentieth of an inch in diameter at the base. These cones, being held by the forceps, or attached to the end of a wire, or even of a splinter of wood, may be directed accurately upon the minutest visible particle; and being a little moistened at the point with saliva, the particle will adhere to the very apex under the strongest blast of the blowpipe.

I conceived that when a very small quantity of paste was used, the extremity of the wire or forceps must necessarily abstract much heat from the fragment under examination, because it must itself be often within the limits of the blue flame; and my object was, as much as possible, to insulate the fragment. These cones need not in fact be more than one-quarter or one-fifth of an inch in length; for so effectually is the conducting property of the mineral substance destroyed, by destroying the continuity of its particles, that one of these cones, of the length of half an inch, may be held at the base by the fingers with impunity, while the apex is in the focus of heat.

One great advantage of this method over the others is, that if fusion ensues, it is owing entirely to the nature of the substance

experimented upon, and not in any degree to the agency of foreign substance acting as fluxes.”—(Annals of the Lyceum of Natural History, New York.)

#### GEOLOGY.

##### 6. *On the Geology of Snowdon.*

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

HAVING observed in No. 24 of the *Annals*, in a paper by Messrs. Phillips and Woods, giving an account of the geology of Snowdon, that the inquiries of those gentlemen were unsuccessfully directed in search of a felspathic rock, which had been mentioned to them as existing in that neighbourhood, I have thought it might not be unacceptable were I to point out where such a rock may be found, namely, close to the town of Caernarvon. Here the rock in question forms a conspicuous craggy knot immediately adjoining the north side of the town, which appears to consist entirely of red felspar, with a large intermixture of quartz, chiefly in the form of separate crystals. This rock is in its appearance so totally distinct from those in the neighbourhood, that I much regret it should have escaped the observation of a competent observer. From a quarry on the south side of the hill, it appears to rest upon the usual slaty rocks so abundant in that part of Wales. C. C. C.

We have to apologize to our correspondent C. C. C. for the delay that has occurred in the publication of his communication. The paper was accidentally mislaid. A scientific friend to whom we showed the specimens which accompanied C. C. C.'s letter, having expressed a doubt as to the rock being felspar, Mr. Children has examined a portion of it to ascertain if it contain potash. By fusion with nitrate of baryta, &c. he readily detected the presence of that alkali.—C. and P.

#### MISCELLANEOUS.

##### 7. *Is the Level of the Baltic stationary?*

About half a century ago, the philosophers in the north of Europe entered into an animated controversy respecting the accuracy of an assertion which even then had become very generally credited, that the water of the Baltic is in a state of progressive diminution. Owing to the want of the necessary data, neither party at that period succeeded in establishing their opinion; but the discussion had the fortunate effect of directing the attention of properly qualified persons to the subject, and of inducing them to observe from time to time the mean height of the water in numerous stations both on the eastern and western coasts. A large body of facts has thus been gradually accumulating, and the results of these coincide with such uniformity in

indicating an actual shrinking of the water,\* that the fact of its being in a state of progressive diminution appears to be now ascertained almost beyond dispute. In the course of the years 1820 and 1821, M. N. Bruncrona, with the assistance of the officers of the Piloting Establishment, and of other individuals familiarly acquainted with the subject, undertook an examination of all the well-authenticated watermarks which have been made along the western shore of the Baltic within the last half century. His results, which are in the fullest extent confirmatory of the conclusions which had been deduced by preceding observers, are consigned in a short memoir in the Swedish Transactions for 1823. The following table exhibits the amount to which, according to his observations, the level of the water has fallen within the last forty years. It is necessary to remark, that, in some instances, the watermarks were much older, and, in others, of a more recent date than forty years: in all such cases, the change which had taken place within the last forty years was estimated by calculating the mean annual depression from the period of the earliest observation.

Province.	Parish.	Place.	Latitude.	Fall of the surface in 40 years in feet.
Vesterbotten.	Bygdea.	Rataskär.	63° 59'	1.50
		Ledskär.	—	2.50
		Ditto.	—	0.50
Gefleborg.	Rogsta.	Balsö.	61 43	2.50
		Hornslandet.	61 37	2.83
	Njutanger.	Agö.	61 32	2.50
	Hille.	Löfgrundet.	61 45	2.50
Stockholm.	Häfverö.	Svarcklubben.	60 11	2.33
	Radmansö.	Gisslinge.	59 46	0.17
		Söderarm.	59 46	2.00
		Sandhamn.	59 17	2.17
	Sorunda.	Landsort.	58 44	1.00
Nyköping.	Bälinge.	Hartsö.	58 42	1.08
		Säfvesund.	58 45	1.17
		Häfringe.	58 35	2.00
Ostergöthland.	St. Nicolai.	Arkö.	58 28	0.67
	Jonsberg.	Kettlö.	58 11	0.83
	Gryt.	Häradoskar.	58 8	1.00
	Loftahammar.	Stedsholmen.	57 50	1.00
Kalmar.	Kalmar.	Skallö.	56 41	0.41
Bleking.		Carlskrona.	56 10	}
		Carlshamn.	56 11	
		Ahus.	55 55	
Christianstad.				0.00
Malmöhus.		Falsterbo.	55 23	}
		Landskrona.	55 22	
Halland.	Onsala.	Hallsund.	57 21	}
Bahusia.	Marstrand.	Koön.	57 53	1.00

Among the many facts which presented themselves in the course of this investigation, and which he considers corroborative of the depression in question, may be mentioned the following:

1. The almost universal remark that the Baltic has become shallower in the ordinary sailing tracts, is admitted by the pilots to be not destitute of foundation, but they are of opinion that this alteration is in general most perceptible in situations exposed to the afflux of driftsand, loose pebbles, or sea-weed, or where the bottom consists of rock. A similar observation has been made in the proximity of the larger towns and fishing stations; for example, according to the chart drawn up in 1771, the mean depth of the sea opposite the harbour of Landskrona was six fathoms; in 1817, it was scarcely five fathoms.

2. Between the numerous islets which are scattered along the coast of Sweden from Haarparanda to the boundaries of Norway, there are sailing tracts where, 80 or 90 years ago, according to the testimony of the oldest and most experienced pilots, the sea admitted ships having a draught of 10 feet, although at present they are impassable to vessels whose draught exceeds two or three feet: at the same time it does not appear that older tracts, in the immediate vicinity of these, have become sensibly shallower.

3. It is asserted also by the pilots, that along the whole coast of Bahusia, the sailing tracts where the bottom consists of rock, undergo a sensible diminution of their depth every 10 years. Many other parts of the Baltic might be mentioned, in which a similar alteration has been taken notice of.

M. C. P. Hallstrom, in a valuable appendix to M. Bruncona's memoir, gives the following table of the corresponding diminution which has been observed in the water of the Gulf of Bothnia.

Place.	Mean height of water		Distance of the water below the original mark, in feet.	Years.	Fall of the water in 100 years, in feet.
	marked	re-observed in			
Raholmen, in the parish of Lower Kalix . . . .	1700	1750	2.05	50	4.10
		1775	2.49	75	3.32
Stor Rebben, in the parish of Pitea . . . . .	1751	1785	1.79	34	5.00
		1796	1.90	45	4.22
Ratan, in the parish of Bygdea . . . . .	1749	1785	2.70	36	4.72
		1795	2.50	46	5.43
		1819	2.69	70	3.47
	1774	1785	0.55	11	5.00
		1795	1.16	21	5.52
		1819	1.60	45	3.57
	1795	1819	0.65	24	2.71
Rönnskar, on the coast of Wasa . . . . .	1755	1797	1.70	42	4.05
		1821	2.87	65	4.35
Wargön, on the coast of Wasa . . . . .	1755	1785	1.45	30	4.83
		1797	1.69	42	4.02
		1821	2.87	65	4.35
Löfgrundet, beyond Gefle . . . . .	1731	1785	2.90	54	5.37
		1796	2.17	65	3.34
Ulfon, in Angermanland	1795	1822	1.58	27	5.85

That the numbers in the last column, however, represent the exact amount of the secular depression, cannot be regarded as fully determined, because it has not yet been satisfactorily ascertained whether the diminution be uniform, or whether it varies at different periods; and whether it is in any respect dependent on local circumstances, or on the climate, or on the state of the atmosphere. Neither has it been ascertained whether this depression, which has been remarked to become less and less from the north of the Baltic towards the south until it finally becomes insensible, diminishes in an equal proportion with the latitude, or whether it follows a different law. Throughout the whole of the Gulf of Bothnia, the fall appears to be uniform, and its amount for the last 100 years appears to be about  $4\frac{1}{4}$  feet; below, at Calmar, it is only 2 feet; but whether the diminution between these two stations takes place in a regular progression is still undecided.

Before concluding, we have only to remark, that this supposed fall of the level of the Baltic must not be regarded as disproved by the issue of the long agitated inquiry, whether the level of that sea is actually higher than that of the German Ocean, and which the observations made of late years at the locks of the canal of Holstein appear to have answered decidedly in the negative. Neither can an *absolute* fall be accounted for by M. Otto's theory, that the sea in one position is gaining in depth exactly what it loses in another; a shifting which he attributes to the banks becoming in some situations more extended, and the bottom more elevated, in consequence of the immense quantities of earth, sand, stones, &c. which are annually carried down by the large rivers which disembogue themselves into the Baltic. Admitting the accuracy of the observations as they are recorded on the rocks themselves, we must also admit either that the rock constituting the bottom of that sea is actually elevated by some subterraneous process, or that its waters are either diminishing by evaporation, or are retiring to some other quarter of the globe. The latter explanation is supported by numerous authentic analogical changes, with which every one at all conversant in the history of the earth is familiarly acquainted; while there are no geological facts at present known which bestow any degree of plausibility upon the former.

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#### ARTICLE XIV.

#### NEW SCIENTIFIC BOOKS. .

##### PREPARING FOR PUBLICATION.

Mr. Bewick is preparing a work on British Fishes, in the style of his preceding volumes on Quadrupeds and Birds.

*Travels in Greece*, by Dr. P. P. Brondsted, will appear in eight Parts, royal 8vo.

A New Scientific Journal is about to be published in Dublin, under the title of the Dublin Philosophical Journal, and Scientific Review, to be published half-yearly.

The Sixth Edition of Dr. Paris's Pharmacologia, in which will be introduced a revolving scale, termed the Medical Dynameter, showing the absolute and relative strength of the different preparations of medicine: in 2 vols. 8vo.

The Mechanic's Encyclopædia, or General Dictionary of Arts, Manufactures, and Practical Science; in 8 vols. post 8vo. with numerous Plates.

An Appendix to Capt. Parry's Second Voyage of Discovery, containing the Natural History, &c.

#### JUST PUBLISHED.

The Transactions of the Geological Society, Vol. I. Part II.

A View of the Present State of the Salmon and Channel Fisheries, comprehending the Natural History and Habits of the Salmon. By J. Cornish, Esq. 8vo. 6s. 6d.

A Compendium of Medical Theory and Practice, founded on Cullen's Nosology. By D. Uwins, MD. 12mo.

An Essay on Curvature and Diseases of the Spine. By R. W. Bampffield, Esq. 8vo. 10s. 6d.

Brewster's Edinburgh Encyclopædia, Vol. 17, Part I. 1l. 1s.

Halkett's Notes on the North American Indians. 10s.

Travels among the Arab Tribes inhabiting the Countries East of Syria and Palestine. By J. S. Buckingham, Esq. 4to. With Illustrations. 3l. 3s.

## ARTICLE XV.

### NEW PATENTS.

J. Gunn, Hart-street, Grosvenor-square, coach-maker, for improvements in wheeled carriages.—Oct. 14.

W. P. Weise, Tooley-street, Southwark, Surry, manufacturer, for improvements in the preparing and making water-proof cloth, and other materials for the manufacturing of hats, bonnets, caps, and wearing apparel, and in manufacturing the same therefrom.—Oct. 14.

H. Marriott, Fleet-street, ironmonger, for an improvement in water-closets.—Oct. 14.

J. Fetlow, Manchester, for improvements in power looms for weaving various articles.—Oct. 14.

H. Mandslay and J. Field, both of Lambeth, engineers, for their method and apparatus for continually changing the water used in boilers for generating steam, particularly applicable to the boilers of steam vessels making long voyages, by preventing the deposition of salt or other substances contained in the water, at the same time retaining the heat, saving fuel, and rendering the boilers more lasting.—Oct. 14.

## ARTICLE XVI.

## METEOROLOGICAL TABLE.

1824.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.
		Max.	Min.	Max.	Min.		
11th Mon.							
Nov. 1	W	29.87	29.82	58	50	—	01
2	S W	29.98	29.87	48	40	—	
3	W	29.98	29.97	48	34	—	
4	N	29.97	29.93	44	32	—	
5	N W	30.24	29.93	46	25	—	
6	W	30.24	30.10	55	25	—	
7	W	30.10	29.87	57	50	—	
8	W	30.12	29.87	50	34	—	
9	N W	30.12	30.04	55	37	—	12
10	W	30.04	29.99	56	50	—	25
11	N W	30.08	29.99	56	42	—	60
12	N W	30.21	30.08	50	40	—	—
13	W	30.21	29.73	55	45	—	25
14	N W	30.06	29.73	50	35	—	03
15	N W	30.37	30.06	45	32	—	—
16	W	30.37	29.98	51	38	—	
17	S W	29.98	29.56	58	52	.95	06
18	S W	29.92	29.56	55	48	—	28
19	S W	29.92	29.68	50	39	—	40
20	S E	29.68	29.61	53	43	—	34
21	W	29.61	29.60	53	40	—	38
22	S W	29.60	28.72	52	42	—	16
23	S	29.13	28.71	52	42	—	15
24	S W	29.48	29.13	52	40	—	
25	W	29.81	29.48	50	31	—	06
26	N W	29.89	29.81	48	30	—	
27	E	29.91	29.84	53	36	.48	
28	S W	29.84	29.38	53	47	—	15
29	S W	29.60	29.38	52	38	—	12
30	S W	29.59	29.46	52	43	.35	46
		30.37	28.17	58	25	1.78	3.82

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

## REMARKS.

*Eleventh Month.*—1. Cloudy. 2—6. Fine. 7. Fine: lunar halo. 8. Cloudy. 9. Fine. 10. Cloudy. 11. Rainy. 12. Fine. 13. Cloudy. 14. Rainy. 15. Cloudy and fine. 16. Fine. 17. Boisterous. 18—20. Rainy. 21. Fine day: rainy night. 22. Showers: windy: very boisterous night. 23. Stormy wind continued to blow furiously all night. 24. Wind a little abated this morning: day fine. 25. Fine day: a heavy shower at half-past five, p. m. 26. Fine. 27. Cloudy. 28. Cloudy: a boisterous wind all night. 29. Fine day: rain at night. 30. Rainy: windy.

## RESULTS.

Winds: N, 1; E, 1; SE, 1; S, 1; SW, 9; W, 10; NW, 7.

Barometer: Mean height

For the month..... 29·813 inches.

For the lunar period, ending the 14th..... 30·002

For 13 days, ending the 1st (moon south)..... 29·881

For 14 days, ending the 15th (moon north)..... 30·031

For 14 days, ending the 29th (moon south)..... 29·613

Thermometer: Mean height

For the month..... 45·616°

For the lunar period..... 46·383

For 30 days, the sun in Scorpio..... 47·050

Evaporation..... 1·78 in.

Rain..... 3·82

*Laboratory, Stratford, Twelfth Month, 21, 1824.*

R. HOWARD.

# ANNALS OF PHILOSOPHY.

FEBRUARY, 1825.

## ARTICLE I.

*On the Life and Writings of Claude-Louis Berthollet.*  
By Mr. Hugh Colquhoun.

(Continued from p. 18.)

IN the course of the investigation into the nature of chlorine, of which the conclusions have just been detailed, Berthollet was induced to examine its action upon the liquid fixed alkalies. In consequence of a very complicated decomposition, the full extent of which it was impossible for him to foresee, the result was a new compound, which is now distinguished by the name of chlorate of potash. This he proved to consist of potash united to an acid composed of muriatic acid and a larger proportion of oxygen than he supposed to exist in chlorine: to this acid he accordingly gave the name of hyperoxygenized muriatic acid. One of the most remarkable properties of this salt is the great proportion of oxygen which it contains. This oxygen is retained, united only by a very weak affinity, and of course is easily disengaged by presenting any of the combustibles with which it readily combines. Berthollet, who was ever the first to foresee the practical application of any of his discoveries, proposed that the new substance should on that account be substituted for nitre in the manufacture of gunpowder. His hint was immediately acted upon, and a manufactory was established at Essone. The effects, however, were lamentable: No sooner had the workmen begun to triturate the mixture of chlorate of potash, sulphur, and charcoal, than the whole exploded with tremendous violence, the building was blown into the air, and several persons perished.

The discovery of this substance has, however, been productive of no small advantage to science. It has tended to complete the theory of chemistry, and has also become an instrument of discovery, extending still farther the limits of our knowledge. Without it, the two oxygen-acids and the two oxides of chlorine had never been known, and cannot yet be prepared; without it,

the chemist would be deprived of an extremely easy and economical mode of preparing the purest oxygen gas ; and without it, Gay-Lussac and Thenard had not been enabled accurately to disclose the ultimate composition of vegetable and animal principles. For although the use of chlorat  of potash for this latter purpose is now superseded by other still more correct and more manageable instruments, yet it constituted the introduction to this branch of chemical analysis, and indeed suggested the employment of the methods more recently adopted.

But the researches of our chemist with respect to the nature of chlorine were attended by yet another result, which has redounded, not less to his own honour, than to the prosperity of France and England. It had been previously remarked by Scheele, that among other extraordinary features, chlorine is characterised by the property of destroying every vegetable colour with which it comes in contact ; and this destruction he found to be not merely apparent but complete. The vegetable colour treated with chlorine cannot be restored by any known chemical reagent : its basis has undergone decomposition. When Berthollet, in the course of his researches, came to the consideration of this property, his attention was peculiarly excited by it ; and in the hope of its throwing some light upon the true constitution of chlorine, he was anxious to examine into its mode of operation. It will be remembered that he conceived erroneously chlorine to be a compound of muriatic acid and oxygen ; and he soon formed a theory conformable to this idea. He held that, since the pouring of an aqueous solution of chlorine upon a vegetable colouring matter, destroys the colour, and leaves only muriatic acid behind ; it is an oxidation of the colouring matter which produces this effect, and the consequence is, that the substance oxidised loses its property of reflecting certain rays of light.

But it is not that Berthollet formed a theory on the subject, which is now a matter of moment ; it is his immediately conceiving that this property of chlorine might *in its application* be made of the greatest practical utility ; and his persevering with a zeal undamped by difficulties till he really rendered it so, that throws an intense interest over this part of his history. Since chlorine destroys vegetable colours completely, he reasonably inferred that it would produce a similar effect upon those substances which injure and obscure the inherent beauty of thread and cloth, and the separation of which is the object of bleaching. He accordingly immersed a piece of unbleached cloth for some time in a solution of chlorine in water, and was extremely gratified to find it come out of a pure white colour. But his mortification was proportionally great on perceiving that cloth so bleached, after a certain time, gradually assumed a dingy yellow colour ;—an alteration which he found greatly accelerated by

treating the cloth with an alkaline ley. Harassed by this impediment, which threatened to take away all permanent benefit from his discovery, he began, however, to conceive that there is a strong analogy between the action of his chlorine upon the cloth, and that of the ordinary process of bleaching, by subjection to light, air, and the moisture of a meadow exposure. He next considered that as the effect of this latter process is not a bleaching of the cloth, but merely such a loosening of the colouring matters as facilitates their final disengagement by some subsequent process, the same result might follow a similar aid given to the bleaching by chlorine. He accordingly conjoined the action of an alkaline ley with the immersion in chlorine, and by subjecting the cloth to these two processes for several times in alternate succession, he was happy enough to succeed in rendering it permanently white. The bleaching was rendered still more perfect by adding the customary finish of the process, a steeping in dilute sulphuric acid, or in very sour buttermilk.

M. Berthollet had brought his new process to this stage of perfection, when he communicated his success to his friend Mr. Watt, the illustrious improver of the steam engine, who happened to be then in Paris. This gentleman, whose whole life was devoted to the advancement of the arts, saw at a glance the advantages that must result to his country by transferring this process from the laboratory of the chemist to the extensive work of the bleacher; and soon after his return to England, he wrote to M. Berthollet, informing him that his father-in-law, Mr. Gregor, proprietor of an extensive bleach field, near Glasgow, had bleached 500 pieces of cloth upon the French system. And it is surely well to mention that our countryman, besides the honour of being the first to carry into England so important a saving of time and expense, in one of the most extensive of the useful arts, had also the merit of inventing and introducing several important modifications of the apparatus required in the operation.

Unhappily for France, she laboured at that time under a system of financial disorder accompanied by heavy taxation, of which that of the gabelle, or tax on salt, was one of the most unequal and of the most severe. And as all the chlorine was extracted from this article, she was unable at once to avail herself of the full benefits which immediately accrued to Britain from this discovery. Besides, the arts had then made comparatively small progress with her; and ignorance, ever hostile to improvement as an innovation, hindered for some time the general diffusion of the new process. Its inherent superiority was, however, too manifest to be slighted by prejudice, and too great to be destroyed by the more formidable obstacle of taxation. It was soon introduced and practised to a large extent by many intelli

gent bleachers who did not follow their business as a mere routine. M. Caillau at St. Quentin, Descroisilles at Rouen, Bonjour at Valenciennes, and Welter at Lille, all men of considerable eminence, who could join science with art, introduced the benefits of the new system into various parts of the empire.

But there are many other processes in the arts besides that of the simple bleaching of dingy cloth, in which the removal of certain colouring matters is advantageous, and which were of course susceptible of great improvement from the application of Berthollet's discovery. One of the earliest and most important of these was first suggested by our chemist himself: it relates to that process in calico printing which is technically known by the appellation of *brightening*. When a piece of calico is dyed with madder, the portions meant to be preserved white are found to have contracted a dull red coloured stain; because, although colouring matters in general cannot form a permanent union with cloth except through the intervention of some mordant, they have often an immediate affinity for the cloth of such strength, as requires considerable labour for their complete removal. The old process for removing such discoloration was to boil the cloth in a mixture of bran and water, and then to expose it to the action of air and moisture for a period of from one to six, or even eight weeks together. This was generally a successful, but always a most tedious, operose and costly system. But the delay, the labour, and the expense, were very greatly reduced by M. Berthollet discovering that a very dilute solution of chlorine destroys colouring matters which are attached to the cloth only by their immediate affinity, while it produces no material alteration on such as are held in combination with it through the agency of a mordant. The new process of brightening by chlorine was of course immediately and universally adopted in all the calico print fields in France, and has now become part of the ordinary routine of that business.

But we are still far from having enumerated even the principal uses to which the bleaching property of chlorine was found applicable. By means of it, Berthollet was enabled to instruct his countrymen in an expeditious and, at the same time, a most accurate method of ascertaining the relative permanency of colouring matters when in combination with cloth. By means of it, Descroisilles brightened turkey red, by destroying the brown colouring matter which contaminates and conceals the beauty of that dye: by means of it, M. le Baron de Born gave a beautiful white to the yellow colour of animal wax, while Berthollet destroyed the green colour of vegetable wax, and gave it the closest resemblance to the bleached wax of de Born: by means of it, M. Chaptal succeeded in removing the stains from old books and prints: by means of it, Loysel bleached coloured

rags for the manufacture of paper ; and to cut short a list which might be extended to a tedious length, M. Berthollet, by means of chlorine, introduced the important improvement of giving to lint and to flax all the appearance of cotton.\*

It would indeed be difficult to mention any one course of investigation which led to so many, so great, and so immediate benefits, as that into the nature and properties of chlorine, instituted and conducted by Berthollet. It often happens, that the author of an important discovery does not live to see it appreciated, and himself acknowledged the benefactor of his species ; but in this case our chemist had the felicity to enjoy the sight of the advantages he had conferred, and even to have many distinguished rivals competing with him in exploring the various uses of which his discoveries were susceptible. Nor should the biographer of Berthollet omit to mention that, notwithstanding his being thus the source of great wealth to his country and to England, he constantly declined to accept of any emolument even from those whose riches had never been amassed but for his researches. All the remuneration that he would receive in return for his benefits was the simple present of a bale of cloth from England, bleached according to his system. Who the merchant was who devised a present at once so delicate and so acceptable, is not now with any certainty known. But it seems fair to conjecture, that it was probably made through the intervention of Berthollet's much esteemed friend Mr. Watt, the first Englishman to whom the process was imparted, and of whom it was as worthy to bestow such a compliment as it was of the French chemist to receive it. Indeed if ever man loved science for her own sake with a pure and sacred ardour, that man was Berthollet ; and he was fortunate in this instance in receiving not only a reward such as no money or power could purchase, but also an immortality such as few men of genius feel assured of. By the universal assent of the French nation, the name of the inventor was adopted into the language

\* This was not one of the least valuable among the practical applications of the bleaching property of chlorine. Lint has neither the suppleness, the elasticity, nor the softness of cotton : its fibre is not so slender or so fine ; it has a glistening appearance, especially after being woven, from which the dull white colour of cotton is exempt, and it has in consequence the property of reflecting the light, so that its whiteness cannot be rendered so perfect as that of cotton cloth. Many attempts had been made to overcome these disadvantages, of which carding and bleaching proved the most substantially useful. Yet even after the most careful performance of both these operations, the approximation of lint in its appearance to cotton, although considerable, was still very imperfect, and the whiteness thus produced was not found to be very permanent. After Berthollet had, however, employed repeated immersions in chlorine, he obtained the gratifying result of approximating lint not only in its appearance, but also in its properties, much more closely to cotton than had been effected by any previous process. The subject was soon after taken up, and some amelioration introduced into the system by Giobert, who explained his mode of operation at considerable detail. Immediately after this, thread and cloth manufactured from lint were thrown into commerce, which it was impossible to distinguish from those made out of cotton.

to designate the new process. The bleaching liquor was styled *lessive de Berthollet*, or *berthollet*; to bleach by means of it was expressed by the verb *bertholler*; the bleachers were named *bertholleurs*; and *berthollerie*, *blanchisserie berthollienne*, *berthollimètre*, were successively adopted to express ideas which a knowledge of the root will at once convey. It is somewhat odd, Descroisilles observes, to find the name of one of the founders of the French *methodical* nomenclature introduced without ceremony to form the basis of a whole class of words, in utter contempt of all its principles.

The nature of the advantages thus introduced was truly surprising. Persons acquainted only with the modern mode of bleaching are astonished when they are informed, that what is now the work of a few days, was formerly the work of a whole summer; that what is now done almost within doors, formerly required extensive tracts of meadow ground at present under the plough; and, finally, that what is now undertaken and accomplished at all periods of the year, was then attempted during only half the year, the wintry period being wholly incompatible with the old process. Nay, in this country the inconveniences relieved by the new system were peculiarly great; for it was by no means uncommon at one time to be at the expense of sending goods all the way to Holland to be subjected to a bleaching process there, whence they only returned after a heavy outlay, at soonest at the expiration of three or four months. The saving of time and of expense to the individual,—the redeeming of so much land to the country,—and in general the activity which has been given to the rapid circulation of capital in the community, have altogether been a source of incalculable benefit to the commerce of England, and to the general comfort of mankind. How happy the man of pure disinterested mind who lived to see himself the author of so many blessings!

Almost every year in the life of Berthollet, after he had arrived at maturity, was productive of a discovery which was either beneficial in extending science, or in promoting some useful art. The series of researches into the nature of chlorine, of which the first fruits were published in 1785, and the application of its properties to so many useful purposes which we have just detailed, exerted the repeated efforts of the investigator during a series of years. That however we may preserve something like chronological order, which is not a little difficult where so many new views start up together in the path, and cross each other in their progress to development, let us return to 1788, the year following that in which the leading views of Berthollet on the nature of chlorine were first given to the world. He had not yet completed his fortieth year, and he rendered it signal by the publication of two memoirs, the first relative to the combination of metallic oxides with alkalies and lime,

and the second detailing his striking discovery of fulminating silver.

It was one of the doctrines commanding the most universal assent at the time, that acids and alkalies are bodies, in each of which there is *indefeasibly inherent a peculiar and distinct nature and principle*. This Berthollet denied. He asserted that the terms acidity and alkalinity convey *no positive and independent meaning*, but imply *mere relation*, and that alone. He said that these words were adopted solely for the purpose of facilitating the explanation of the affinities of certain classes of bodies for each other, and that the same substance which acts as an *alkali* when united with one body, may act as an *acid* when it forms an union with some other. And he referred for the truth of this opinion to the strong fact that the same body, *oxide of lead*, not only, in combination with all the substances usually termed acids, *acts as an alkali*, but also in union with potash, soda, ammonia, and lime, forms in some cases crystallizable compounds, possessing all the properties of salts, and consequently *acts as an acid*. This doctrine, however, notwithstanding its own merit, and the fame, now universal, of its author, met with no favour from men of science, although, of late years, the labours of Davy and Berzelius have forced its correctness upon the understandings of nearly all the chemical world.

In the remarkable discovery which Berthollet at this time made of a new compound which has been named fulminating silver, although we have no particular account of the circumstances attending its first formation, yet we are sure from its nature he must have incurred imminent danger. Had he then met the fate which so many have since encountered from handling this substance, and perished on the threshold of discovery, the loss to science must have been nearly irreparable.

Fulminating silver, by the application of a low heat, by the slightest agitation, by a brush from a feather, or the pressure of a fallen drop of water, instantly explodes with tremendous violence. By detonation in a strong metallic tube, Berthollet found the products to be, the silver reduced to its metallic state, azote, and water. Before its discovery, no analogous compound was known, except that of fulminating gold; and the numerous experiments made on that subject by various chemists during 35 years, proved successful in adding only two others to the list.

In the year 1791, Berthollet conferred one of his most signal benefits upon his country by the publication of his work, entitled "*Elements of the Art of Dyeing*." France in general, and the neighbourhood of Lyons and Rouen in particular, had long been celebrated for the assiduity and success with which the art of dyeing had been there cultivated. Ever since the days of Colbert, whose zeal for the prosperity of his country is

undoubted, and whose views of political economy, though sadly narrowed by the prejudices of his day, were nevertheless honestly and earnestly directed to the encouragement of manufacturing and agricultural industry, ever since his days it had been the practice in France to appoint one of her most eminent chemists to the peculiar superintendence of the processes of dyeing. After the death of Macquer, it will be recollected that Berthollet was his successor in this situation, and it must surely have been a source of great pleasure to a mind constituted like his, to find that an enlightened view of the processes of dyeing, adapted to the advanced state of chemical science, was greatly wanted; and to feel that he was fully able to satisfy the wishes and fulfil the expectations of his country.

From the time of the publication of M. d'Albo, under Colbert in 1669 down to 1784, when Berthollet was appointed, a succession of distinguished chemists, Dufay, Hellot, and Macquer, each filling this honourable situation in their turn, presented to the French dyers an excellent digest of the principles and practice of their art, in which the improvements of science and of method were alike explained, and placed on a level with the advancement of knowledge and the arts. The last of these chemists, Macquer, though he lived till 1784, had not assented to the new theory of Lavoisier, and had indeed only in the last years of his life employed some of its doctrines to an extent so partial, as rather to increase the perplexity of the old system than materially to remove it. Accordingly, the vacant situation had not long been occupied by Berthollet, when the revolution in chemistry, and the many important discoveries accompanying it, rendered every preceding work on dyeing defective in information, and to the modern dyer almost unintelligible in its explanations. These deficiencies were amply supplied, and the various processes of the art were ably explained according to the new philosophy of chemistry, by the publication of Berthollet's *Elements of Dyeing* in 1791. This was indeed the work of a master, and bears throughout, in a striking manner, the impress of its author's character. It is alike remarkable for the profoundness, the truth, and the originality of its general views of the processes in the art; and by a happy application of correct theory, the work abounds in new and valuable information respecting the nature of colouring matters, mordants, and every substance used in dyeing, which is accompanied with a copious explanation of the most advantageous methods of employing them in practice. Thirteen years after this, another greatly enlarged edition of this work, embodying every improvement, was prepared by the joint labours of Berthollet and his son, A. B. Berthollet, then a young man of the fairest promise.

Among the other improvements which Berthollet introduced

into the art of dyeing, there is one of so great importance, and whose investigation at the same time involved such difficulties, that it deserves particular notice. This is his mode of employing prussian blue in the formation of the brightest permanent blues and greens now in use.

The remarkable brilliancy of this substance as a colouring matter, early recommended it to the notice of chemists and practical dyers, all of whom, however, were baffled in their attempts to discover any means of fixing it in an equal and permanent manner upon cloth. Menon, Macquer, and Roland, successively assayed this task, and each proposed a separate process for performing it; but in practice the same ill success attended the proposals of each. In some, only a very pale shade of colour was produced; in others, where the colour was at once deeper and moderately permanent, it was always found unequally distributed over the cloth; and in a third, the colour, which at first was bright and equable enough, was at the same time so fugitive, from its having been applied in a manner merely mechanical, that a slight wearing speedily injured it, and after a few washings it almost entirely disappeared.

The mode in which Berthollet overcame all these difficulties was a most ingenious one, and it was the result only of much research and experiment on his part; in the course of which he was aided by the celebrated calico-printer Widmer. It occurred to Berthollet that, as prussian blue is a compound substance, of which one constituent by itself has a strong affinity for cloth, it might be possible that the other constituent should unite readily with the first, even on finding it previously combined with the cloth, although the compound body so presented refuses any such union. He, therefore, first treated cloth with oxide of iron, one constituent of prussian blue, for which the cloth has a powerful affinity, and next superinduced upon the whole the acid principle, by the application of an alkaline prussiate. The acid colouring matter, uniting with the oxide of iron, formed the dye, without at all disengaging the previous combination between the cloth and the oxide. The prussian blue communicated in this manner is found to have nearly as strong an affinity for cloth as the oxide of iron has when in separate combination with it; and thus, by the result of this method, a blue colour of the greatest brilliancy and permanency was added to the art of dyeing.

In employing the same substance as an ingredient to produce a green, M. Berthollet's ingenuity was again severely tried, and again it overcame every difficulty. To achieve this, there is one other step necessary in the series of affinities, which are all brought into play only by the order in which they are made to follow each other; a process which in the end produces a com-

pound combination, refused to every other system. Green is a colour which the dyer invariably produces by the mixture of blue and yellow; but the prussian blue has *no affinity for cloth*, and when this obstacle is overcome, it has *no affinity for the yellow colouring matter*; neither has the yellow any direct affinity for the cloth; yet there must be a combination of these colours with *each other* and with *the cloth*, or there can be no use made of prussian blue in dyeing green. To effect this, the three constituents of the green colour were applied separately. First, the cloth was treated with oxide of iron, for which it has a great affinity: the next step was to add to this a yellow dyestuff, with which also the oxide has a tendency to combine; and, lastly, there was superinduced above all the prussic colouring matter. The result was, that the oxide of iron, once combined with the cloth, retained united to *itself* the *yellow* and the *acid colouring matters*, forming as the product a beautiful and lasting green. This process is now in universal practice among dyers and calico-printers; and however great the extent of its use, the pleasure of seeing it universally diffused was here also the sole return that ever was made to the author for his perseverance and ingenuity. Men, like Berthollet, sometimes confer benefits too great to be remunerated by any other than the rich reward of their country's gratitude.

We have now arrived at one of the most important and instructive periods in the history of nations, the era of the French revolution. It is true, that in tracing out the life of Berthollet, we have heard nothing of those distractions which had agitated the government of the country for years previously, we have seen nothing of its financial distress, of its frequent changes of ministry, of the assumption of supreme power by little more than one house only of the States General, of the violence offered to the King's family, of their restraint, imprisonment, and trial; nor need we look upon the fearful sight of that sun of 'royalty, which had so long been the vital principle of France, sinking, as it seemed, for ever, in an ocean of blood. It is unnecessary to give an account of these events, for important as they were, they dragged not forth Berthollet from the bosom of science, where he continued to prosecute his uninterrupted researches. He was not, however, destined, even as a chemist and man of science, long to remain unoccupied by, or unmingled with, the busy ferment of the politics of the day.

The circumstances that followed the erection of the revolutionary government are of a kind not soon to be forgotten. It will be remembered that France was then, by common consent, put under the ban of every other civilized nation. To aid her, or to hold intercourse with her, was alike forbidden under the severest penalties; while ultimate success was held to be

secure to those who thought to tame the national spirit by withholding every means of defence.

Nor were these expectations of the States who laid France under their ban by any means ill-grounded. For that country, naturally rich, while she produced abundance of grain at an easy rate, and her wines returned her a large and certain revenue that was reaped without difficulty, had nevertheless allowed herself to fall far behind several neighbouring kingdoms in many of the most useful arts. She had of course accustomed herself to rely on the intervention of commerce for procuring to her many of those articles of comfort, which soon became the necessities of civilised life. When, therefore, she was considered a proscribed nation; when her former princes at Coblenz with her refugees, aided by the House of Austria with her allies, locked her in on the side of Europe, and the fleets of Britain swept every sea, and blockaded every port, France was obliged to recoil upon her own resources. And on turning homeward for that which was now denied her from abroad, she found resources capable in time of high improvement, but the demand was for *immediate assistance*, and the salvation of the state depended upon its being *immediately furnished*. Arts were not here ready to be *improved* on the instant;—the very foundation of arts was wanting. Manufactures, the soul of resource, were scarcely known, or if known, were nowhere found to exist among this hitherto agricultural and commercial community. In so awful a crisis, the eager nation called upon her men of science to come forth from their seclusion and retreats, to impart their knowledge, and become the instructors of a willing people. And the call was answered. Then those individuals who knew nought, save the theory of an art, found every where pupils to whom a hint sufficed, and new arts sprung up at once, and flourished at the invocation of science. Former processes were improved and abridged; new resources were discovered; new manufactures were invented; and at the moment when it seemed that France must fall a defenceless prey to her aggressors, she arose armed to the combat. At first, it is true, ere these changes were fully developed, her foes made some impression upon her, for the attack was sudden indeed; but even when a hostile army was for a short time within forty leagues of the capital, not a man relaxed for one moment from his exertions;—not a citizen thought of deserting his country. And full soon, to the astonishment of the world, France displayed her new-found resources, her foes perished round her impenetrable frontier, and she in her turn becoming the assailant, tamed her continental enemies, and dictated to them their terms of submission in the heart of their own conquered capitals.

Of all these changes, of which science was the original source, Berthollet, and his illustrious friend Monge, were the heart and soul. At the same time, it is but justice to add, that they had many illustrious associates, who, like them, unacquainted with any thing but science and theory, became like them the active instructors of their countrymen in every art. It followed as a natural consequence of their zeal and of the importance of their services, that no private body of men enjoyed more of the public confidence than the chemists of France. From the aid they had lent to an administration fluctuating amid the struggles of party, and young in the art of governing, they naturally and of necessity became possessed of no inconsiderable political influence. And never was any trust discharged with more faithfulness, ability, and moderation. It was at this period that most of the great and liberal institutions of which France is now so proud, were organised.

The state of desperate exigency from which Berthollet and his associates were enabled to raise France, can with difficulty be now fully appreciated: let us take one single instance in illustration of it. Although so warlike a country, she had been in the habit of importing all her saltpetre. When this necessary article was denied her, and an instant invasion imposed the appalling demand of gunpowder, to the amount of many millions of pounds, was made as essential to her safety in this crisis, a committee of the most eminent chemists was applied to, and the country soon received as the result of their investigations, the delightful intelligence, that an inexhaustible supply of saltpetre, easily accessible, lay within the bosom of their native soil. "In five days," one of the committee boldly affirmed, "in five days after the saltpetre shall have been extracted from the earth, gunpowder manufactured from it shall charge your cannon:" and his words were verified to the letter.

It was then that the whole face of the country seemed for a time covered with manufactories of this substance. The citizens emulated each other in amassing and lixiviating the proper soil. Berthollet and the chemists rivalled each other in hastening from department to department, to teach the best mode of extracting the salt; and such improvements were thus introduced, that very soon processes were completed in France in a few hours, which then cost other nations the labour of a month. The result of the whole was an abundant supply of gunpowder for the French camps and fleets, while their arsenals and magazines were stored with ammunition; and the extraction of saltpetre from the soil continues at this day a permanent source of productive employment of the national capital and industry.

Another scarcely less important benefit was at this time conferred on France by her men of science, which seems too much

connected with this subject and with our chemist, to be omitted here. There was an urgent demand for cannon, musquets, sabres, &c. to provide and equip one million of men, who were eager to take the field, but wanted arms. The ordinary manufacturer was unable to meet a demand so extensive; and besides, the fabrication of steel, and even of the finer kinds of common iron, was unknown to him. Here again the French philosophers came forward. A committee was appointed, of which Berthollet and Monge were the leading members, remarkable alike for their talents and for their unwearied exertions; valuable improvements were introduced into the smelting and purification of iron; a profound investigation was instituted and completed of the processes by which that metal may be converted into steel; and the immediate result was, that the people became instructed; the nation's wants were supplied for the time, and extensive permanent establishments of that difficult manufacture were formed in various parts of the country.

It was not only, however, by their talents, nor yet by their ingenuity, that Berthollet and his friends were enabled to aid and to enlighten their country. Occasions presented themselves in which their integrity, and that rare quality of civil courage, were not less conspicuous. It was this, indeed, which greatly contributed to give them at first the perfect public esteem and confidence which they long enjoyed. During the reign of terror, a short time before the ninth Thermidor, when the system was a favourite one of raising up pretended plots to give pretexts for fresh ravages of the guillotine, a hasty notice was given in a certain sitting of the committee of public safety, that a conspiracy had just been discovered to destroy the soldiers, by poisoning the brandy which was ready to be served out to them just previous to an engagement. It was said that the sick in the hospitals who had tasted this brandy all perished in consequence of it. Immediately, orders to arrest all those implicated in suspicion, or rather those previously marked for execution, were issued, and numbers in chains awaited their doom. To Berthollet it was referred to analyse this liquor; he was at the same time made fully aware that Robespierre *would* have a conspiracy, and all knew that opposition to the will of that monster was generally death. Having finished his analysis, Berthollet drew up his results in a Report, which he accompanied with a written explanation of his views, and he there stated in the plainest language the simple truth, that there was nothing very detrimental mingled with the brandy, but that it was merely diluted by water holding small particles of slate in suspension—an ingredient which filtration would speedily render innoxious. This Report deranged the plans of the Committee of Public Safety, who accordingly sent for the author to convince him of

the inaccuracy of his analysis, and to persuade him to alter its results. Finding that he remained unshaken in his opinion, "How, Sir!" exclaimed Robespierre, "darest thou affirm that muddy brandy to be free from poison?" Berthollet immediately filtrated a glass of it, and in his presence drank it off. "Thou art daring, Sir, to drink that liquor," said again the ferocious President of the Committee; "I dared much more," replied Berthollet, "when I signed my name to that Report." This was indeed to take the hungry lion by the beard, and it is probable that a revolutionary tribunal would soon have rewarded his integrity, were it not that the same shield which defended the physician of Louis XI, protected also the life of our chemist. The knowledge of each was necessary to the existence of the tyrants whom they had the misfortune to serve.

During the early years of the revolution, we find Berthollet employed by his country in many important public situations. In 1792, he was named one of the Commissioners of the Mint, into the processes of which he introduced considerable improvement; in 1794, he was appointed a member of the Commission of Agriculture and the Arts; and in the course of the same year he was chosen Professor of Chemistry at the Polytechnic School, and also in the Normal School. In these situations, however, it must be confessed that his mode of communicating his views was not adapted to the level of a general audience. He was too apt to presuppose a degree of knowledge or talent in those listening to him, which it is vain to expect in any public audience; and of course the Professor dwelt too little upon elementary explanation and detail. A teacher should suit his discourse at least to the ordinary average of mind which he is called upon to instruct, and if he commence in a strain too high, his hearers are not carried along with him as he unfolds his views. It is on this account that men of the greatest genius have frequently been the least successful instructors, and it is certain that the faults just mentioned accompanied the lectures of M. Berthollet.

The same year is remarkable in the life of Berthollet and in the history of science, the intimate connexion between which we have often already had occasion to remark, by the establishment of the celebrated *Annales de Chimie*, a work to which, from the first, he has been a principal contributor. This is a journal which, ever since its formation, has continued so distinguished for the number of its original and important memoirs, that it has yet no rival amid all the hundred scientific periodicals now publishing in Europe. To supply such memoirs as these was not indeed the ostensible purpose of its institution, but, as the Introduction informs us, to communicate to the chemists of France the progress of the science throughout Europe. It is extremely

probable too, that it was intended to be a powerful instrument in diffusing and establishing the principles of the modern system of chemistry. The original authors were Lavoisier, Berthollet, Monge, Fourcroy, Guyton de Morveau, Dietrich, Hassenfratz, and Adet.

In 1795, at the organization of the Institute, which now embraces every man of any talent or celebrity in France, we find M. Berthollet taking the most active lead, and the records of that Institute afford abundant evidence of the perseverance and assiduity with which he laboured for its interests. Of the committees, to which, as is the custom, all original memoirs are in the first place referred, we find Berthollet oftener than almost every other person, a member, and his signature to the Report of each work stands generally first.

But indeed the zeal of M. Berthollet in the interests of science, and his anxiety to diffuse widely the truest principles by means of the press, seem to have been wholly unremitting; for we find him not only connected, as we have just mentioned, with the establishment of journals for that end, but even looking into foreign nations, whose scientific works he always read, to select those publications among them, the translation of which into the French tongue might most advance science and benefit his country. Accordingly we find him in 1788 engaged in the translation of Kirwan's Essay on Phlogiston, and supplying it with notes of his own, in the sole view of correcting those errors which that work without such an antidote might spread. And in the same spirit, though from a different motive, we again find him, in 1798, superintending M. Riffault's translation of Thomson's Chemistry, adding his own notes to it, and bringing the work under the immediate notice of his countrymen by prefixing to it an Introduction. That Berthollet expected this translation to prove of eminent service to the chemists of France, is a great compliment to our countryman, and that he was right in so expecting is well proved by the same gentleman, M. Riffault, once more translating, according to its new arrangement, the fifth edition of the Doctor's work, in 1818.

The translation of each of these works was eminently useful to science, although they were ushered into the notice of French philosophers under very different auspices. The first was accompanied by notes, refuting every one of its doctrines, and was translated that it might be overthrown: the second was accompanied by notes and an introduction, elucidating the system and supplying whatever seemed defective; and this work was translated that it might become the manual of the French chemist. Kirwan was a man who had made many chemical discoveries, some of them of considerable importance, and he was besides possessed of the power of arguing most ingeniously,

accompanied, as that quality not unfrequently is, by a proneness in the heat of argument to advance propositions which were not altogether rested on the most solid basis. He remained the most illustrious disciple of the old school, and he published his *Essay* with the express view of defending the doctrine of Phlogiston, after he had superinduced upon it several modifications, which seemed to give it a certain adaptation to the progress of modern science. The refutation of this *Essay* seemed, therefore, to the French chemists to be the destruction of the last antagonist worthy of their notice. Berthollet, accordingly, in conjunction with Lavoisier, Fourcroy, Morveau, and Monge, taking the translation of the *Essay*, section by section, annexed to it a refutation in which the principles of the old and new schools were contrasted, and the latter triumphantly established on the ruins of the former. Lavoisier's share was the Introduction and three sections, Berthollet took three sections more, Fourcroy took also three, Morveau two, and Monge one. Never was any refutation more complete; as indeed Kirwan himself was among the first to admit.

In respect to the other translation with which Berthollet was connected, the motives which dictated to him the interest he took in the work, were precisely the same, yet his treatment of it, as has been already observed, was the very opposite. Nor indeed could Berthollet at that time have given to the chemists of his country a more acceptable and useful present than was Thomson's *System of Chemistry*, accompanied by his own notes, and furnished with an Introduction from his own pen. This work, by far the most successful of its kind which had then been attempted, was selected under the circumstances just mentioned by Berthollet, because (as he states in the Introduction), it is unrivalled as a *Thesaurus* of every known fact of importance connected with chemistry, and as containing the most accurate account of the history of every known substance. Indeed from the extreme regularity and methodical precision characterizing the work, from the cool discrimination with which every subject is weighed and treated of according to its relative importance, and from the accurate historical detail prevailing throughout, there is at this moment no system of the science of chemistry in which so complete and extended information on every topic is to be found organized and detailed as in Thomson's *Chemistry*; which is evident from its having already reached the sixth large impression; and from its having long ago passed into the laboratory of the Frenchman and the German on the continent of Europe, and of the Armenian in Asia; while it has been reprinted for the use of the American student.

*(To be concluded in our next.)*

## ARTICLE II.

*On the Climate of the Antediluvian World, and its Independence of Solar Influence; and on the Formation of Granite.* By Sir Alexander Crichton, Knight, St. W. FRS. &c.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

*Harley-street, Cavendish-square, Jan. 4, 1825.*

THE proofs of the climate of the antediluvian globe having been for a long period of time independent of solar heat are numerous, and well known as detached geological facts; yet they have not, so far as I know, been brought together and presented under this very interesting point of view, nor has the principle been applied to the explanation of such geological phenomena as it is calculated to elucidate. They have attracted notice indeed, as characterizing epochs in the stratification of the earth, and as exhibiting a succession of events in the great work of creation, but the light they throw on the climate of the world in its first ages, has not arrested that degree of attention which a subject so interesting seems to demand.

The view which I am going to take of the temperature of the antediluvian world, of its independence of solar heat, and its gradual changes, is founded on the most indisputable facts; and the conclusions to be drawn from them are so highly important in themselves, and so intimately connected with the study of geology, that the subject appears a fit one for a scientific journal, where it will probably awaken the attention of many readers, and lead to further researches.

In the following pages it will not be necessary to enter into very minute details, but to turn the attention of geologists to certain great classes of facts as data which will at once be acknowledged to bear immediately on the subject. Many others will naturally present themselves to every well informed and reflecting mind.

Among the earliest proofs which geology offers of the antediluvian climate having been independent of solar influence for a great length of time, are those circumstances which show that all the surface of the earth, from a very high northern latitude to a corresponding southern one, was of one uniform temperature, and *that* a very high one, when compared with the temperature of the same range of latitude in our days.

I do not allude to the fact of fossil remains of elephants, rhinoceroses, hyenas, and other animals of warm climates, having been found in northern latitudes; for the migratory disposition of animals on the one hand, and on the other the possibility of their having been transported after death from distant regions,

against which opinion I shall have much to offer afterwards, cause me to exclude them at present as uncertain witnesses ; neither do I admit as proofs of a high temperature in the northern latitudes of the antediluvian world those fossil shells which are found in the limestone rocks of northern countries ; for though many of them bear a close analogy to those which are at present found in the Indian and South Pacific Oceans, yet both Brocchi and Olivi have shown that the shells of the Indian Ocean are also found in very temperate climates, for instance, in the Mediterranean Sea ; and it is most probable that all the shells of the Sub-Appenines were inhabitants of that sea, since there is a great resemblance between them and the living genera. Although these, therefore, are to be rejected as positive proofs of a very elevated temperature in northern latitudes at the time that the inhabitants of these shells were alive, yet they may be admitted as concomitant proofs of a great equality of temperature, and that a warm one, over a great portion of our earth such as cannot be explained by solar influence ; for when we reflect that the analogous species of several of these (such as the *nautilus pompilius* found at Grignon and Courtagnon) are only found in very warm climates, and that a fossil shell analogous to the living *trochus agglutinans* which inhabits the seas of South America, has also been found as far north as Hordwell and Barton, in Great Britain ; at Grignon, in France, and also in the contemporaneous deposits of many other places in Europe, it follows as a most probable supposition, that the temperature of those northern latitudes was many degrees warmer formerly than it is at present. Whoever reflects that among the immense number of fossil shells many are remarkable for their extreme thinness, delicacy, and minuteness of parts, none of which have been injured, but, on the contrary, are most perfectly preserved, will find it impossible to admit the notion of their having been brought from warmer and distant regions to the places where they are found by some great and sweeping catastrophe. Many of them could not have been carried even a short distance by an agitated ocean, or the retreat of waters, without suffering attrition and fracture.

If they are met with composing the mass of entire mountains, in the interior of continents, and far above the level of the sea, this only proves either that the strata in which they are found was raised above the level of the sea after their death, by some subterraneous and extraordinary force, or that the regions in which they are found were abandoned by seas which formerly covered the places in which they are now detected.

When we search deeper in the bowels of the earth than the strata in which the bones and skeletons of the large terrestrial quadrupeds of warm climates are found, or shells analogous to those of the South Sea, we arrive at a very peculiar and interesting Flora, which must arrest our attention for some time.

The perfect state in which most of the plants belonging to the coal formation are found, is calculated to do away all idea of their having been brought from distant regions by powerful currents, or by the retreat of waters. Their leaves, many of which are of the most slender and delicate structure, are found fully expanded, and in their natural position in regard to the rest of the plant, and laid out, as it were, with as much care as if in the *hortus siccus* of a botanist. The minutest parts do not appear to have suffered attrition or injury of any kind. Those persons who have not had the benefit of consulting extensive collections of geology may see the proofs of this assertion in the plates of Knorr, Schlottheim,\* Sternberg,† Parkington, and Brogniart,‡ and in those belonging to the numerous monographs on fossil plants, which are to be found in the Transactions of learned societies.

It is quite impossible to reconcile the many facts of this kind with the effects of any sudden or violent change of place, or with a long journey however gentle. Compare the calm deposit of shells and the appearances of the still calmer death of the antediluvian vegetable world, with the boulder stones, the gravel, and the disjointed, dispersed, and fractured osteology of the diluvian deposits, and it will be allowed that there is not the slightest analogy between these classes of events.§ The fossil plants of some of the lowest strata, such as those which belong to the oldest coal formations, are either of the fern tribe, or they are arborescent monocotyledonous plants resembling palms, or as M. Adolphus Brogniart has justly remarked upon the authority of M. Descandolles, dracenas yuccas and pandanus.

But it is acknowledged that the living plants which have the nearest resemblance to these antediluvians, are tropical plants which have not yet been found beyond the 39th or 40th degree of north latitude. Every coal country in every part of the world which has hitherto been examined, abounds in the fossil remains of similar vegetables; and it may be remarked in the very outset of this essay, that as certain plants, perhaps I might say all plants, belong to specific temperatures, or at least depend for their life and health on heat much more than on soil, and as most of the remains of plants belonging to the coal formation appear from their integrity to have been buried where they grew, we are forced to admit the conclusion, that wherever they are found, there must have been a warm temperature.

The laws of vegetable life as relating to temperature are positive, and, therefore, when connected with the individuals of the

\* Die petrifactenkunde.

† Versuch einer Geognostisch—botanischen Darstellung der Flora der Vorwelt.

‡ Sur la Classification et Distribution des Vegetaux Fossiles. •

§ On this subject consult Blumenbach's Specimen Archaeologiæ Telluris, &c. and Breisbach's Instituzione Geologica.

antediluvian vegetables, they throw the greatest and surest light on the subject of its climate.

That the similarity of any two Floras depends more on a similarity of temperature than of soil, appears from a multitude of facts. The *Arnica Montana*, for instance, is found on the low marshy lands which border the Baltic, whereas in the south of Europe it is only found on mountains, whence its specific name. The *Betula nana* which is found on Mount Jura is met with in Lapland at the foot of the mountains. The *Betula Alba*, or common birch of this country, is found in the plains of Scotland and of Russia, but in Portugal it only grows on the mountains. When we come to plants of the same genus, but which are not of the very same species, we find a similar analogy taking place as demonstrative of the influence of climate; thus many gentians, and many of the pine and larch tribes, grow on the mountains of South America as well as on the Alps, but not in the low plains, much less the valleys. In many of the high plains of Columbia, and almost under the equator, apple and willow trees, and common furze, are found, while in the valleys under the same parallel are found palms, &c. The plants of the North American Flora which are most analogous to those of the Flora of Europe, are found in analogous temperatures.

Although distant regions are often found to possess the same temperature during a great part of the year, yet a multitude of circumstances, independently of the three great co-ordinates, latitude, longitude, and elevation, may produce varieties of climate, all of which we are incapable of enumerating, such as the vicinity to extensive lakes or seas, the height and propinquity of mountains, the extent and direction of the land and sea, winds, &c.; and this explains why any two places under the same parallel and elevation, and which have apparently similar climates, may yet differ considerably in their effects on vegetable life, and thus favour the growth of some new species. It is probably owing to such causes that the plants of the Cape of Good Hope and those of New Holland are not similar, although their climates resemble each other in many respects.

Among the fossil remains, however, of the plants which belong to the coal formation, we scarcely find any variety, let the latitude, longitude, or elevation, be what they may; but supposing a few species were discovered in any one district which were not common to all, it would only prove the influence of a local cause, the rest being all alike. Almost all genera and species of plants belonging to that early period of the world, appear to have been extremely limited; they are remarkable for their similarity under whatever parallels they are found.

Every plant in the present world, independently of its natural dwelling-place, has, as it were, a central spot in which it flourishes best; and considering this spot as the centre of a

circle, or rather as a zone, the plant degenerates in proportion as it approaches the limits of this district. This kind of zone seems to depend chiefly on the elevation above the sea, and consequently on temperature. Some plants descend from the mountains towards the plains, others creep upwards to a limited height, and then disappear. But in the ancient world, any difference which might be supposed to have existed in regard to the elevation of those places which are called coal basins, did not produce a variety in the plants of that age, which is another proof that a cause of heat was then acting on the earth, which did not resemble the action of the sun in our days.

It has been remarked, that the fossil remains of the vegetable world which are found connected with the coal formation, are all of them similar to plants requiring great heat and moisture, and many facts in geology induce us to believe, that at those early periods of our earth, there was less dry land than at present. The primitive and transition mountains, together with the carboniferous limestone, appear to have been the only formations which preceded the life of those plants which are found connected with coal. The organic remains of the limestone, on which the coal reposes, show that it had long been under water, and consequently demonstrate both the extent and elevation of the waters at that period, and, therefore, the waters themselves would naturally serve as a vehicle for distributing the germs or seeds of antediluvian plants over the greatest possible extent; hence another cause of the similarity of the Flora of these early times in every part of the world. If washed by currents, or wafted by winds to distant shores, where they found a similar climate, they would grow; but at present the seed vessels of South American plants, collected sometimes on the coast of Norway, perish. It seems superfluous to multiply arguments to prove this position, since every horticulturist who rears exotic plants, knows well from experience how much of his success depends on giving them the degree of heat they require.

Except, therefore, we admit that vegetable life was under totally different laws from what it is at present, we must allow that a much greater uniformity of temperature existed in the early ages of the world over the whole globe, than is the case in our days. There is in fact no way of accounting for the very little variety which exists in the antediluvian plants of the period I am alluding to, and of their great similarity in every part of the world, but on the principle of great extent and uniformity of a high temperature, however difficult it may be to reconcile this to our notions of the obliquity of the earth and solar influence.

But there is a difference at present of at least  $41^{\circ}$  of heat (mean temperature) between the parallels in which coal has been discovered. Between these, the diversity in the genera and species of plants at present is very great, so much so indeed that

there is no resemblance between the Floras of the two extreme points. At the time, however, of the coal formation, the Flora of these two remote parallels was the same, both as to genera and species.

If it be allowed that a variety in climate and soil are the two chief circumstances which occasion the greatest variety in the vegetable kingdom; and if it be allowed that the plants of the coal formation and of the most ancient strata were all of the simplest structure, and almost entirely belonging to the acotyledonous and monocotyledonous tribes, we have another proof of the uniformity of temperature and soil at that period over an immense extent of the earth. The more complicated vegetables, those of the dicotyledonous kind, do not appear until a much later period,\* when the cause of the uniformity of temperature of the ancient world was gradually becoming less and less, and dying away, and the sun beginning to take an ascendancy over a cause of heat which had until then exerted supreme influence, and which appears to have belonged solely to the earth itself.

Whatever the temperature may have been which was necessary to support the life of the vegetable kingdom of that early period of the earth's existence, it must be admitted that that temperature was the same towards the polar regions as in the tropical ones, for in both, the genera and species of antediluvian plants are similar, and the shells and corals of the mountain limestone in the most distant parts of the contemporaneous strata also correspond with each other. In the collection of the Geological Society of London, there is a specimen of a very remarkable variety of *felicites* from the coal formation of Australia, about the 29° south of the equator, and another exactly resembling it from the coal formation of Newfoundland in the 49° north of the equator. The fossil shells of Van Dieman's Land correspond with those of Derbyshire. Upon descending below the coal formation, proofs of the equality of a high temperature over the whole earth are multiplied; for upon examining the mountain, and more especially the transition limestone, which comes more immediately in contact with the primitive rocks, we find madrepores, encrinites, corallites, and all the varied habitation of sea polyps, the existing *analogues* of which are always found in tropical climates. It is in the Pacific Ocean, and chiefly in the Red Sea, the Persian Gulph, and the Caribbean Sea, that the greatest coral rocks of modern times are found. But in the ancient world, not only pentacrinites, madrepores, corallites, and encrinites, are found in the transition and mountain limestone of the coldest regions, but also whole genera of testacea, the living resemblances to which, with a few exceptions, are only to be met with at present in warm climates.

\* In the Whitby coal which lies over the oolite.

It is well known that the sensible heat of our atmosphere varies with the latitude, longitude, and the elevation of the place where the observation is made, and that the temperature on the surface of the earth corresponds in a great degree with that of the atmosphere; but the ancient temperature of the earth appears to have been equal and permanent in every spot, at least for a very long period.

Observations made in mines prove that the heat of the earth increases with the depth. Several well-authenticated facts will immediately be brought forward to substantiate this position; but before doing so, it may be remarked that we have another series of observations which have not been investigated with all the attention they merit, leading to the same conclusion, and which it is highly interesting to examine under this point of view,—I mean the temperature of springs as they rise at different depths, or under particular strata.

As connected with the present subject of inquiry, springs exhibit two distinct set of phenomena; first, the constancy of their temperature under all varieties of seasons; and, secondly, the difference of their respective temperatures as they rise from different depths. Some interesting papers on the constancy of temperature in a great number of springs, are to be found dispersed in the Transactions of learned societies. To such an extent does the central cause of heat counteract the agency of the seasons, that mineral springs which rise at no great depth remain of an almost uniform temperature throughout the year. The temperature of one near Berlin similarly situated was examined at different periods by two very accurate observers, Wahlenberg and Erman. The former found that the heat of the source did not vary more than 0·25 of Reaumur from August to the month of April following. Erman, in a subsequent series of observations, did not find it to vary more than 0·05, and he ascribes the difference of the results to the greater accuracy of his instruments.—(See the *Abhandlungen der Königlichen Akademie der Wissenschaften von Berlin für 1819.*)

The deeper the sources of tepid and hot springs are, so much hotter in general is the water which rises from them. The tepid springs of Matlock and Buxton rise in the immediate vicinity of amygdaloid and basaltic rocks, and hotter springs seem to come from still greater depths.\* The celebrated and learned Humboldt says, that the hot springs in various parts of South

\* Mr Cordier found the temperature of a mineral spring at Cantal, which takes its rise from the granite, to be a little above the heat of boiling water (+ 100 of the centigrade thermometer). But a more astonishing fact is related by Mr. Link, who states, that the heat of the mineral source at Caldas was + 150 of the centigrade thermometer. The hot baths of Montiegas at the foot of Sierra de Estrella, and all the hot springs in Portugal; those of Vals near Aubenas, in the Department of Ardèche; those of Weldband, near Salzburg, are thrown up either by granite or gneiss.—See Brogniart's *Mineralogy*, vol. i. Article, Water.)

America arise from the granitic and primary strata. As to the hypothesis of their deriving their heat from the chemical decomposition of sulphurets, &c. the limited and changeable operation of such a cause, compared with the permanency and greatness of the effect, are sufficiently strong reasons to make us abandon this explanation.

It is here that I must add a few facts which relate to the heat of mines. The following are taken from a paper of Robert Balguy, Esq. in the *Edinburgh Philosophical Journal*, vol. vi.:—

*Whitehaven Colliery, Cumberland.*

Average temperature of a spring at the surface . . . . .	49 Fahr.
Ditto of water at the depth of 480 feet. . . . .	60
Air at the same depth . . . . .	63
Air at 600 feet . . . . .	65

*Workington Colliery, Cumberland.*

A spring at the surface . . . . .	48
Water at the depth of 180 feet . . . . .	50
Ditto at the depth of 504 feet below the level of ocean, and under the Irish sea. . . . .	60

*Teem Colliery, Durham.*

Water at the depth of 444 feet . . . . .	61
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*Percy Mine Colliery, Northumberland.*

Average temperature of water at the surface. . . . .	49
Water 900 feet deeper than the level of the sea. . . . .	68
Difference . . . . .	19

*Jarrow Colliery, Durham.*

Average temperature of water at the surface . . . . .	49
Water at 832 feet . . . . .	68

*Killingworth Colliery, Northumberland (being the deepest Coal Mine in Great Britain).*

Water at the surface . . . . .	49
Air at 790 feet deep . . . . .	51
Ditto at 900 from the surface, after having traversed 1½ mile from the downcast pit . . . . .	70
Water at the great depth of 1200 feet. . . . .	74

Baron Humboldt, whose talents for observation, and whose accuracy, cannot be doubted, informs us that the mine of Valenciana is so warm, that the miners are constantly exposed to a temperature of 91·4 of Fahrenheit, while the mean temperature of the external air is 60·8.

The springs which issue from veins of the same mine at the depth of 1638 feet, have a temperature of 98·2, which is 5·4

warmer than the air of levels in which the miners work ; and this fact is of itself, when added to Mr. Bald's observations on the water in mines, sufficient to set at rest, for ever, the supposition of the heat being owing to the miners, their horses and lights, &c. The health of a miner requires a constant circulation of air, which renders the heat of mines more remarkable.

The average temperature of air at the mouth of the mine

of Reyas, near that of Valenciana, was ..... 69·4

Air at the depth of 630 feet . . . . . 92·7

Mr. Bald very properly remarks, that the heat of coal mines cannot arise from the decomposition of sulphurets, for these never suffer decomposition *in situ* ; if they did, the greater part of the coal mines in the world would have been destroyed by spontaneous ignition. In the mina Purgatoria, the height of which above the level of the sea is equal to the Pic of Teneriffe, the air in the mine was 67·3 Fahrenheit.

From the foregoing observations, it is evident that the elevation of a mine above the level of the sea does not regulate its temperature as it does that of the surface. Water at the depth of 1200 feet under the sea in the Killingworth Colliery, was stated to be 74° Fahr. ; while the air at 436 feet deep in the mine of Villapenda, in Mexico, and which is more than 3000 feet above the level of the sea, is 84·9.

When the phenomena of the antediluvian Flora, and the laws of vegetable life, are considered in connection with all that has been adduced, we are necessarily led to the same conclusion to which many celebrated geologists have arrived, partly from taking a different road of inquiry, and partly from conjecture ; namely, that there is a source of heat in the centre of the earth itself which must be referred to, as the cause of the uniformity of temperature of the ancient world.

In regard to the first of these suppositions, it is most certain that when the granitic crust is duly considered in all its analogies, it is much more reasonable to consider it as a crystallization arising from fire than as a crystalline deposit from a watery solution. We have no proof that any fluid, such as water, is capable of holding such an immense quantity of the most insoluble of all substances in solution, and indeed it is probable that the waters which were destined to act so remarkable a part on the surface of our globe were, in the beginning of time, of the purest kind, having no saline or mineral contents whatever to deposit. The experiments of Sir James Hall and others have proved that earthy substances, when fused under great pressure, are capable of taking on a crystalline texture ; and observation demonstrates that even when not under great pressure, the elements of feldspar, mica, amphibene, hornblende, pyroxene, anal-

cime, and various other bodies, when fused by the heat of a volcano, unite to form these compounds, most of which appear as perfect and beautiful crystals in the very substance and cavities of the fused mass. Lavas, basalts, volcanic pitchstone, porphyries, &c. are full of such crystallized bodies, and throw a light by analogy on the formation of granite, inasmuch as they demonstrate the positive fact, that these crystalline substances, bearing a close resemblance to the ingredients of this rock, may be formed by igneous fusion; and when to this is added the results of Mr. Micherlich's most ingenious experiments on the artificial production of pyroxene and mica by fusion, the evidence becomes almost complete.

In the very substance and cavities of lavas, we meet with amphotene, harmatome, feldspar, icespar, Thomsouite, aragonite, mica, amphibole, and augite, all in a crystallized state. It, therefore, appears probable, that these crystalline bodies were formed when the liquid lava allowed their elements to arrange themselves according to their affinities. To suppose the central part of the earth a mass of highly ignited liquid matter still existing in a state of fusion is not consistent with any thing that we know; but as the brilliant discoveries of Sir Humphry Davy in chemistry have demonstrated beyond the possibility of doubt that all the earths are metallic oxides, it is not incongruous to suppose that the nucleus of the earth was *in toto*, and still is in part, in a completely metallic state, and that the granite crust of the earth was formed by a general and contemporaneous oxidation and consequent ignition of the whole of its surface. This doctrine would account in a natural manner for the earthy and alkaline oxides which are found in all the rocks and minerals which we suppose to be of igneous origin, or, in other words, for all those substances which have till of late been considered as distinct earths and alkalies. It accounts not only for the universality of the granite involucrum, but also for the similarity of its composition; for in fact, the granite is to be considered as a mass of earthy oxides which were produced by the action of air and water, or watery vapours, on the metallic mass. When we reflect for a moment on the intense heat produced by the rapid oxidation of a very few grains of potassium or sodium; we may conceive, if imagination can go so far, the more intense heat of this globe during the simultaneous conflagration of the whole of its surface. What a state of chaos and disorder, from which was to spring a series of secondary causes, the agency of which gave birth to a succession of others, each operating for a time, and thus accounting for the whole order of the superstructure.

We must suppose the presence of water and atmosphere to explain the oxygenation of the metallic mass, and it is conform-

able to reason to admit that the great First Cause which distributed through the immensity of space the primordia of so many worlds, would employ the simplest, and at the same time the most effectual means for accomplishing the ultimate purpose and end. There is no necessity to imagine an ocean already formed full of saline parts which held the earths in solution, and which it was to deposit by subsequent evaporation. The purer the element, the more rapid and effectual would its first action be; but then as a necessary result, a crystallized coat being thus formed, a stop was put to the further conflagration and oxidation of the metallic nucleus, except in a few spots where rents and fissures occurred, which would admit either water or air to the central mass. The time was now arrived at which the elementary water became fully saturated with every kind of soluble oxide, whether earthy or alkaline. Its impregnation with these bodies, therefore, was the immediate consequence of its first action on the metallic mass, and its subsequent deposits can be accounted for as a series of natural events.

Before proceeding any further, I think it right to state, that this hypothesis concerning the cause of the central heat, was first started, as far as my reading goes, by James Smithson, Esq. who, in a short introduction to a paper delivered to the Royal Society on the Analysis of a Saline Substance from Vesuvius, published in vol. 103, part 2, of the Transactions of that Society, advanced the opinion as being founded on Sir H. Davy's discoveries; he appears to have been satisfied with merely throwing out the idea, and to have totally abandoned its development. Mr. Smithson's opinion and the grounds for it are so shortly but correctly expressed, that I request permission to insert them here.

"The existence (says Mr. S.) in the skies of planetary bodies which seem to be actually burning, and the appearances of original fire discernible on our globe, I have conceived to be mutually corroborative of each other; and at the same time when no answers could be given to the most essential objections to the hypothesis, the mass of facts in favour of it fully justified, I thought, the inference, that our habitation is an extinct comet or sun."

"The mighty difficulties which formerly assailed this opinion, great modern discoveries have dissipated. Acquainted now that the bases of alkalies and earths are metals eminently oxidable, we are no longer embarrassed either for the pabulum of the inflammation, or to account for the products of it."

"In the primitive strata, we behold the result of the combustion. In them we see the oxide collected on the surface of the calcining mass, first melted by the heat, then by its increase arresting further combination, and extinguishing the fires which generated it, and, in fine, becoming solid and crystallized

over the metallic ball." Mr. Smithson then adds, that he considers, as I also do, the metallic nucleus which remains enclosed as the source of volcanos, and considering the high interest which attaches itself to their ejections, proceeds to the chemical analysis of the saline substance which forms the subject of his paper.

Having done justice to the opinion of this learned and excellent chemist, I must observe that the notion of our planet having ever been either a comet or sun, is not only an unnecessary postulate, but a most improbable conjecture. Every observation made on comets strengthens the suspicion, that so far from being burning bodies, they are masses of transparent fluid having very little density; and a sun, according to the received definition, being the centre of a system, cannot be a fit denomination for our earth.

This doctrine has also been adopted by M. V. Buch. Whether it suggested itself to his mind as an original idea, I know not; but as he does not mention its concordance with the discovery of Sir H. Davy or Mr. Smithson's hypothesis, I presume it must. That the opinion of so celebrated, experienced, acute, and sensible a geologist as M. V. Buch, must have great weight with all who are acquainted with his excellent writings, needs no comment.—(See his paper on Basaltic Islands in the *Abhandlungen der Königlichen Gesellschaft der Wissenschaften von Berlin*, Baud, iii.)

The inferences which Mr. Micherlich draws from his ingenious and successful attempts to produce crystallized minerals by heat, lead him to a similar doctrine. He says, "The artificial production of minerals by fusion puts beyond doubt the idea of our primitive mountains having been originally in a state of igneous fusion. This state gives a satisfactory explanation of the form of the earth, of the increase of temperature at great depths, of hot springs, and many other phenomena. At that time, during this high degree of temperature, the waters of the sea must have formed an elastic fluid around the globe, according to the experiments of M. Cagnard de la Tour."

(To be continued.)

## ARTICLE III.

*Meteorological Table kept at Bushey Heath in 1824.*

By Col. Beaufoy, FRS.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

THE barometer and thermometer were observed at nine o'clock in the morning, at which hour the temperature of the external air is nearly the same as the mean temperature; see Columns 3 and 8.

The coldest day was Jan. 14, thermometer 22°; and the hottest Sept. 2, thermometer 82°.

Months	Barom.	Ther.	Rain.	Evap.	Six's.		Mean.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Var.
					Least.	Great.										
	Inches.		Inches.	Inches.												
Jan..	29.629	36.6	0.794	1.85	34.1	41.3	37.7	2	3	0	0	3	9	1	13	0
Feb..	29.331	37.1	7.734	0.95	35.0	43.2	39.1	0	9	4	3	0	8	3	2	0
March.	29.319	38.8	1.880	2.07	35.1	45.4	40.2	1	5	0	2	0	12	0	11	0
April..	29.386	44.3	2.116	3.32	38.5	51.4	44.9	0	9	1	4	0	8	1	7	0
May..	29.497	50.8	3.850	3.09	45.2	57.0	51.1	0	10	1	2	0	4	3	8	3
June..	29.438	55.6	5.074	3.19	49.9	62.9	56.4	1	11	1	5	0	6	0	5	1
July..	29.657	61.6	1.698	4.42	55.2	69.7	62.5	2	2	5	2	0	10	2	7	1
August	29.441	60.3	2.125	3.31	54.7	64.5	59.6	0	7	1	3	1	11	3	2	0
Sept..	29.108	57.6	3.663	2.69	53.1	61.6	58.8	2	5	1	5	0	12	1	4	0
Oct..	29.179	49.3	3.107	1.63	45.9	54.8	50.3	0	2	2	8	0	14	2	3	0
Nov..	29.118	45.4	3.110	1.46	41.6	50.4	46.2	0	1	1	0	0	17	5	6	0
Dec..	29.332	40.2	2.782	—	36.2	45.2	41.0	0	3	0	1	0	18	4	5	0
	29.290	48.1	32.933	—	43.7	51.2	48.9	8	67	17	35	4	129	28	73	5

June 24, the greatest degree of heat was 53°

Dec. 25, thermometer 54°.

## ARTICLE IV.

*On the Mathematical Principles of Chemical Philosophy.*

By the Rev. J. B. Emmett.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

Great Ouseburn, Nov. 11, 1824.

IN several papers which have been made public through the medium of your journal, I have endeavoured to investigate some of the principles of chemical science, which are chiefly of a mechanical nature, and to show the agreement of the mechanical

laws of corpuscular action with the Newtonian philosophy. In the present communication, more obscure phenomena, in which the agency of electricity, caloric, and attraction, are concerned, come under examination.

The relative magnitudes of the particles of matter, and the ratio of their forces of attraction, must first be determined. If the centripetal force of a particle of matter belong to its surface only, which I have supposed in the former communications, the weight of a particle of matter =  $F \times D^2$  {D being the diameter of the particle, F its force of attraction}.

Therefore, atomic weight {W} =  $F \times D^2$  ..... (a)

Also  $F = \frac{W}{D^2}$  ..... (b)

And  $D = \sqrt{\frac{W}{F}}$  ..... (c)

If the centripetal force be competent to the entire particle, the following formulæ result :

$W = F \times D^3$  ..... (1)

Also  $F = \frac{W}{D^3}$  ..... (2)

And  $D = \sqrt[3]{\frac{W}{F}}$  ..... (3)

Under a given volume, the number of particles is as  $\frac{1}{D^3}$ ; the surface of each particle is as  $D^2$ ; therefore, the quantity of surface contained in a solid of given magnitude is as  $\frac{1}{D}$ ; hence, upon the first supposition, the weights of equal volumes, or specific gravity {S} is as  $\frac{F}{D}$ .

$\therefore S = \frac{F}{D}$  ..... (d)

Also  $F = S \cdot D$  ..... (e)

And  $D = \frac{F}{S}$  ..... (f)

But upon the second supposition,  $F = S \dots$  (4)

Make (b) = (e),

Then  $\frac{W}{D^2} = S \cdot D \therefore D = \sqrt[3]{\frac{W}{S}}$  ..... (A)

Make (2) = (4),

And  $\frac{W}{D^3} = S \therefore D = \sqrt[3]{\frac{W}{S}}$  ..... (5)

In each case, therefore, the diameter of a particle of a solid is as the cube root of the atomic weight, divided by that of the specific gravity. This is only an approximation to the truth; for the particles have to be supposed similarly situated; and we

do not possess means of ascertaining their relative positions; the corrections to be applied must be furnished by a knowledge of the laws of chemical action; so that this department of science is similarly situated with several parts of physical astronomy, in which the anomalies can be ascertained, and proper corrections made, only by means of formulæ derived from the primary laws.

Make  $(c) = (f')$ ,

$$\text{Then } \sqrt{\frac{W}{F}} = \frac{F}{S} \therefore F = \sqrt[3]{\{S^2 \times W\}} \dots\dots (B)$$

By help of the above formulæ, and others which are easily investigated, many properties of solids may be ascertained.

These are properties which depend upon that force which produces the phenomena of gravitation; and in order to ascertain to what extent this force is concerned in producing chemical changes, or what relation it bears to those forces which are conspicuous in producing chemical actions, we must compare the results of experiment with the conclusions deduced from the above formulæ.

The atomic diameters of the following solids are calculated from (A).

	Atomic diameter.	Atomic weight.	According to
Gold . . . . .	5.039 . . . . .	24.838	Berzelius.
Gold . . . . .	4.061 . . . . .	*12.9	Brande.
Silver. . . . .	5.013 . . . . .	*13.3	Davy.
Silver. . . . .	6.341 . . . . .	26.88	Berzelius.
Copper . . . . .	4.497 . . . . .	*8.00	Davy.
Copper . . . . .	3.556 . . . . .	4.00	Wollaston.
Iron . . . . .	4.448 . . . . .	*6.85	Davy.
Iron . . . . .	3.530 . . . . .	3.45	Wollaston.
Lead . . . . .	5.943 . . . . .	24.9	Davy.
Lead . . . . .	4.848 . . . . .	*12.95	Wollaston.
Tin. . . . .	4.641 . . . . .	*7.31	Davy.
Tin. . . . .	5.848 . . . . .	14.7	Berzelius.
Zinc . . . . .	3.979 . . . . .	*4.4	Davy.
Zinc . . . . .	4.848 . . . . .	8.0	Berzelius.
Phosphorus..	4.217 . . . . .	1.335	Davy.
Phosphorus..	4.61 . . . . .	1.74	Wollaston.
Sulphur. . . . .	4.641 . . . . .	2.00	Wollaston.
Carbon . . . . .	6.979 . . . . .	0.223	Sp. gr.
Carbon . . . . .	3.634 . . . . .	1.526	Sp. gr. "

In this table, I have calculated from several sets of atomic numbers; for different chemists of equal eminence assign to the atoms of solids very different weights. I have chosen the extremes, as it would be but tedious, and not at all satisfactory, to give the diameters calculated from many numbers differing

only in decimals, without the means of knowing which are the true values.

All chemists agree in the atomic number of carbon, but its specific gravity is unknown: that of different charcoals is very various; I have, therefore, calculated the diameter of the atom of carbon from the greatest and least gravity of wood charcoal. The diamond might be substituted, but since it has never been proved to be the pure carbonaceous element, such substitution would be premature.

In the following table, I have calculated the forces of attraction {F} by formula (e);  $F = D \cdot S$ . I give the numbers as they result from the multiplication, &c. of the different tables now in use; when the same can be extended to gaseous and liquid bodies, oxygen or hydrogen may be made the unit in all tables.

		Atomic weight.
Gold, F.....	=97.2527	24.838
Gold . . . . .	78.3773	*12.9
Silver. . . . .	52.6365	*13.3
Silver. . . . .	66.5805	26.88
Copper . . . . .	39.5736	*8.00
Copper . . . . .	31.2928	4.00
Iron . . . . .	34.6054	*6.85
Iron . . . . .	27.4634	3.45
Lead . . . . .	67.4530	24.9
Lead . . . . .	55.0248	*12.95
Tin. . . . .	33.8793	*7.31
Tin. . . . .	42.6904	14.7
Zinc . . . . .	27.853	*4.4
Zinc . . . . .	33.936	8.00
Phosphorus . .	7.464	1.335
Phosphorus . .	8.1597	1.74
Sulphur. . . . .	9.2355	2.00
Carbon . . . . .	1.556	0.223 Sp. gr.
Carbon . . . . .	1.545	1.516 Sp. gr.

On the second supposition; i. e. that the density remaining the same, the weight of a particle of matter is as the cube of its diameter, F will be proportional to the specific gravity of each solid.

In these tables, the numbers marked with an asterisk are from the same tables as those of the gases and compounds in Table 4.

From this table, the most inflammable solids appear to have the least force, or the least tendency to the earth. The order (taking the numbers marked with the asterisk, and which seem generally very consistent), is carbon, phosphorus, sulphur, zinc,

tin, iron, copper, silver, lead, gold, which appears the precise order of inflammability, except with regard to lead, which should precede silver.

The attraction for oxygen follows the same order; carbon decomposes the phosphoric and sulphuric acids, and all the metallic oxides; zinc precipitates all the metals below it in a metallic state; tin and iron precipitate copper and the metals below it; copper precipitates silver and gold. Also the adherence of oxygen to the bases is the same; heat alone decomposes the oxides of gold and silver; but the oxygen is generally separated with greater difficulty, as the metal is more remote from gold in the table.

Also those bodies which are capable of combining chemically are attracted to the opposite poles of the galvanic battery; this is supposed to arise from an electric energy belonging to every particle of matter, and combination is explained upon the principles of electrical attraction and repulsion (whether such electrical energies exist cannot, perhaps, be proved at present; however, the term, electric energy, may be used with propriety, until one can be devised which is free from hypothetical views; at present by electric energy, I mean simply to denote the fact, that the particles of bodies have determinate tendencies to the poles of the galvanic series, which differ in intensity in the bodies which tend to the same pole). Oxygen always tends to the positive pole, and appears to have the highest negative energy of all known bodies. If then we refer the inflammable bodies to oxygen, the most highly inflammable will differ most in their electric energy from it; i. e. the most inflammable bodies have the highest positive energy, or are most vigorously attracted by the negative pole. It appears from the table, that those bodies which have the smallest force of gravitation {F} are most remote from oxygen in their electric state, or are the most highly positive: the order will be as follows, each substance being more highly positive than those which follow it; 1. Carbon; 2. Phosphorus; 3. Sulphur; 4. Zinc; 5. Tin; 6. Iron; 7. Copper; 8. Silver; 9. Lead; 10. Gold. In this list, the errors are not greater than might be expected; for we cannot assume any table of atomic numbers to be critically correct; besides, for want of better data, the particles of all solids must be supposed to be similarly situated; but I have demonstrated in a former paper, that the order of arrangement, whilst the particles remain in contact, may produce a change of one-fourth of the entire volume, therefore one-fourth of the specific gravity; however, since all the metals are fusible, the variation cannot amount to nearly this quantity in any case. The results, however, are sufficiently exact to show, that the most inflammable or most highly positive substances, have the least tendency to the earth. Or if the second supposition be made, the same results nearly;

for the specific gravity of zinc is 7; tin 7.3; iron 7.78; copper 8.8; silver 10.5; lead 11.3; gold 19.3. With regard to carbon, phosphorus, and sulphur, perhaps there may be some doubt; however, they appear to be more highly positive than the metals; for (Phil. Trans. 1807) Sir H. Davy has proved that when a polished metallic plate is separated from contact with sulphur, the sulphur is positive, and the metal negative. Phosphorus entirely precipitates most, if not all the metals from their acid solutions. Charcoal precipitates many by the assistance of light. From these circumstances, these bodies appear to possess a higher positive energy than the metals have. Should these data be proved to be correct, the following deductions may be supposed to be rendered highly probable:—

1. The most inflammable solids have the least tendency to the earth, or the least density.

2. Those solids which have the greatest attraction for oxygen, have the least tendency to the earth, or the least density.

3. The most inflammable solids, or those which have the least tendency to the earth, are the most highly electro-positive.

Inflammability arises from the greatness of the attraction of a substance for oxygen (supposing the latter the supporter of combustion), and this force is proportional to the difference of their electrical energies; and the latter bears an evident relation to the force of gravitation. Some philosophers suppose the particles of every body to possess an invariable electrical state, to which chemical attraction is ascribed; all corpuscular attraction has been ascribed to it: if this be the case, no solid can be simple; for bodies equally electrified with the same power seem to repel, and certainly do not attract each other; every simple or elementary body must be gaseous; the cohesion of solids and adhesion of liquids, on this hypothesis, must be owing to the attraction existing between the different intensities of dissimilar particles; therefore all solids and liquids must be compounds, which certainly has not been proved; nor can it be disproved in the present state of science. Should this hypothesis be proved correct, would it not appear that electrical energy and attraction of gravitation are the same power? That the united energies of the particles of terrestrial matter constitute its attraction, and that the most highly positive bodies (which appear to have the least tendency to the earth) approach most nearly to its mean energy? Should this hypothesis be proved to be correct, from the manifest connexion between the powers, which has been pointed out, these queries appear as if they would be answered in the affirmative. In the explanation of the phenomena, however, there is no absolute necessity to assume the existence of electric energies; for electricity may be regarded as a foreign agent; we may suppose, and upon very good grounds, that it acts only as a decomposing power, and that those bodies which have

the greatest tendency to the earth, have the greatest tendency to the positive pole of the galvanic battery, and the contrary. In this case, the attraction of gravitation may be the sole force which produces chemical attraction, as well as cohesion, capillary attraction, adhesion of fluids, &c.; and that when the galvanic power is applied in effecting decomposition, bodies possessing the greatest force of gravity attach themselves to the positive, and those having the least to the negative pole. Upon this hypothesis, electricity is not an agent in producing attraction; but, like caloric, its action is regulated by the attraction of gravitation according to some determinate law, acting primarily as a power opposed to attraction. Although highly important, it is impossible to decide between these hypotheses, in the present state of chemical science: the principal reason is, that we are totally ignorant of that which we denominate the electric fluid; whether it is a fluid or power *sui generis*, or a modification of others, is unknown; it has the power of attraction, perhaps of repulsion, and when accumulated, it either produces, conveys, or excites heat; electric phenomena may be produced separate from chemical action, as is the case in a large electric column, or galvanic battery, charged with pure water; but whenever it produces chemical changes, heat is excited, and decomposition goes forward in the battery; yet electricity excited by the common electrical machine has the power of decomposition, and excites heat. These facts render it very doubtful whether what we denominate the electric fluid is the principal agent in producing chemical changes; however all phenomena may be equally explained, and all investigations carried forward without having recourse to any hypothetical views, by making observed facts the basis of future research, viz. that the most inflammable solids have the least tendency to the earth (F), and the strongest determination to the negative pole of the galvanic series.

In cases of simple combustion, it has been observed generally, that those bodies which have the lightest atoms, i. e. in which the ratio of the oxygen to the base is the greatest, evolve the greatest quantity of heat during combustion; the analogy may be clearly traced; but if the forces of attraction (F) be compared, those bases, whose force is the least, usually evolve most heat: the following table exhibits some cases:—

When hydrogen is combined with oxygen,				
the Oxygen	Base	:: 7.5 :	1.0 ....	F. base unknown.
	Potassium	:: 7.5 :	37.5 ....	0.65?
	Sulphur	:: 7.5 :	15.0 ....	0.86
	Carbon	:: 7.5 :	5.7 ....	0.89?
	Iron	:: 7.5 :	52.0 ....	3.2
	Gold	:: 7.5 :	97.0 ....	7.2
	Mercury	:: 7.5 :	190.0 ....	7.6

This is only an approximation, an analogy which may hereafter lead to important results ; for the quantity of heat evolved depends upon the quantities of heat contained in the bodies, and the quantity remaining in the compound, which will generally be greater when it is a gas or liquid, than a liquid or solid. The union of bases with chlorine, iodine, sulphur, and some others, present analogous phenomena of combustion, and the same analogy may be clearly traced.

Since the atomic diameters of potassium and of carbon are uncertain, their force is doubtful ; the former certainly is capable of existing in a state of much greater density than it possesses in its metallic state, as the great density of pure potash demonstrates : the real density of carbon is also unknown ; it certainly is much greater than is generally supposed ; for when the lightest charcoal in fine powder, or lamp-black, is perfectly mixed with water, and boiled so as to expel all the air contained in the interstices, it rapidly sinks in the liquid, even if a considerable quantity of gum, or saline matter, be contained ; yet chemists state its gravity to be about 0.223. Besides, the method by which the specific gravity of porous solids is usually found introduces very great errors ; for by reason of capillary action, the mercury employed will never enter the pores and interstices of porous solids ; besides the capillary interstices of all light porous solids are filled with air in a considerable state of condensation, which keeps the mercury at a considerable distance from contact with the solid ; even water does not readily enter. When the specific gravity of a light porous solid has to be taken, it should be immersed in water, or any suitable liquid except mercury, and either boiled or exposed to a vacuum for some time, by which means it will be freed from air : the error introduced by allowing the air to remain, makes the gravity of a charcoal, in reality heavier than water, only .223.

In all analytical researches, chemical tables should be computed for weights and magnitudes proportional to those of the particles of bodies ; for in combination and decomposition, the quantities are proportional to the atomic weights ; an ultimate atom possesses all the properties which belong to a body, and the capacity for heat is as the capacity of one particle multiplied into the number of particles ; therefore the capacity of an atom of any body is the real representative of the capacity of that substance ; the expansion of a body being caused by the separation of its particles, the separation which takes place between two adjacent particles is the true ratio of the expansion. The same may be extended to all other tables. In the following table, I have computed the atomic capacities of a number of substances. The atomic capacity = capacity of a given weight  $\times$  atomic weight. The former numbers are taken from Dalton's

System of Chemical Philosophy; the atomic weights, from Brande's Manual; the weight of oxygen is 1.

	Atomic capacity.	Atomic weight.
Hydrogen . . . . .	2·85	
Oxygen . . . . .	4·75	
Carbonic acid . . . . .	0·414	
Azote . . . . .	1·37	
Aqueous vapour . . . . .	1·75	
Water . . . . .	1·13	
Ice . . . . .	1·02	
Lime . . . . .	1·06	
Carbonate of lime . . . . .	1·7	
Hydrate of lime . . . . .	1·16	
Litharge . . . . .	0·754	
Red lead . . . . .	0·849	
Carbonate of lead . . . . .	1·112	
Vitrified oxide of lead . . . . .	0·68	
Oxide of tin . . . . .	0·822	
Oxide of zinc . . . . .	0·74	
Brown oxide of copper . . . . .	2·27	
Oxide of antimony . . . . .	1·817	
Red oxide of iron . . . . .	1·66	
Gold . . . . .	1·2415	24·83
Gold . . . . .	0·645	*12·9
Silver . . . . .	1·064	*13·3
Silver . . . . .	2·15	26·88
Mercury . . . . .	1·00	—
Copper . . . . .	0·88	*8·00
Copper . . . . .	0·44	4·00
Iron . . . . .	0·89	*6·85
Iron . . . . .	0·448	3·45
Lead . . . . .	0·996	24·9
Lead . . . . .	0·518	*12·95
Tin . . . . .	0·5117	*7·31
Tin . . . . .	1·029	14·7
Zinc . . . . .	0·44	*4·4
Zinc . . . . .	0·8	8·0
Sulphur . . . . .	0·38	2·0
Carbon . . . . .	0·197	
Nickel . . . . .	0·37	
Antimony . . . . .	0·36	
Bismuth . . . . .	0·35	

In this table, the asterisk denotes the atomic number which is contained in the table whence the atoms of the other substances are derived.

The table represents the ratios of the real capacities of bodies

for heat; and if we knew the ratios of the absolute quantity of heat contained in bodies, we should be able to estimate the real quantity evolved during combustion and chemical changes; however as the heat evolved or absorbed depends primarily upon capacity, the table furnishes approximations which probably do not differ much from the truth, and leads to several important conclusions. An example or two will show its application.

Atomic capacity of carbon. . . . . = 0.197  
+ capacity of two atoms of oxygen. . . = 9.500

9.697  
— atomic capacity, carbonic acid . . . . . 0.414

∴ caloric evolved by the combustion of one  
atom of carbon . . . . . = 9.283

Atomic capacity of hydrogen . . . . . = 2.85  
+ atomic capacity of oxygen. . . . . = 4.75

7.60  
— atomic capacity of aqueous vapour. . = 1.75

∴ caloric evolved by the combustion of one  
atom of hydrogen . . . . . = 5.85

Atomic capacity of carbonic acid. . . . = 0.414  
+ atomic capacity of lime. . . . . = 1.06

1.474  
— atomic capacity of carbonate of lime = 1.700

— 0.226

or 0.226 of caloric are absorbed.

From the table, the most inflammable bodies appear to have the smallest capacities, and may therefore be supposed to contain the smallest quantity of caloric: thus oxygen has the greatest capacity: the capacity of hydrogen is large, when compared with that of the metals; since, however, it is highly elastic, its capacity must be much greater than it would be if hydrogen were reduced to the solid state. Gold, silver, and mercury, have a larger capacity than copper; copper than iron, tin, or zinc; these larger than that of sulphur or carbon. Hence those bodies which possess the highest electro-negative energy, or are attracted most powerfully by the positive pole, or which have the greatest tendency to the earth, have the greatest attraction for caloric. These facts may be applied in investigation without reference to any hypothetical views. In compounds

containing oxygen, the atomic capacity is usually greater than that of the base; for instance, the oxides of tin, lead, copper, &c. have a greater capacity than the metals themselves possess; the increase is not in proportion to the quantity of oxygen: the reason is, that oxygen does not exist in the same state of density in all solid oxides.

$$\left. \begin{array}{l} \text{If } m = \text{weight of base} \\ n = \text{weight of oxygen} \end{array} \right\} \text{in a compound.}$$

$$a = \text{sp. gr. base.}$$

$$b = \text{sp. gr. oxygen as it exists in the compound.}$$

$$c = \text{sp. gr. compound.}$$

$$b = \frac{n \cdot a \cdot c}{(m + n) \cdot a \cdot m \cdot c} = \text{sp. gr. of the oxygen required.}$$

Calculating according to this formula, the sp. gr. of oxygen in glass of antimony is 2.21; in phosphoric acid 5.1?; in oxide of arsenic 1.4; in red lead 3.2; in black oxide of manganese 3.1 or 2.7; in red copper ore 1.47; in iron mica 1.36. Now the most highly electro-negative bodies, or those solids which have the greatest tendency to the earth, have the greatest attraction for caloric, and the least for oxygen: therefore their capacities for heat are the greatest, and the oxygen is retained with the least force; therefore in the most highly electro-negative combustibles, the oxygen retains more caloric than in the electro-positive, is most easily disengaged, and their oxides act powerfully as supporters of combustion. Thus the oxides of gold, silver, mercury, peroxides of lead and manganese, easily inflame phosphorus, oxygenate sulphur, and produce other effects which prove that the oxygen retains very much caloric, and it is so easily disengaged, that the three first are reduced by heat alone, and the other two, by the same treatment, part with one atom of oxygen; while the oxides of iron, tin, zinc, and particularly of potassium, calcium, hydrogen, and other highly electro-positive inflammables, produce no such effects, and are reduced with difficulty, requiring the assistance of other inflammables in addition to heat. When we are possessed of accurate tables of the electrical powers of all the bodies which are supposed to be simple, and of the primary compounds of the capacities for heat, more accurate than any at present existing, and of the true specific gravities, we may expect to arrive at conclusions highly important to science, and which will establish chemical philosophy upon a mathematical basis: at present we cannot expect more than the developement of some of the primary laws of action.

The atomic expansions of solids are in the order of their fusibilities; they are exhibited in the following table: the atomic expansion = expansion of equal lengths  $\propto$  atomic diameter.

	Atomic expansion.
Iron . . . . .	0.5634
Gold . . . . .	0.5685
Copper . . . . .	0.7644
Silver . . . . .	1.0427
Tin . . . . .	1.1600
Zinc . . . . .	1.234
Lead . . . . .	1.389

In this table, the atomic diameters are those marked with the asterisk in Table 1. The order corresponds with that of their fusibilities as nearly as can be expected; for finding the expansion of the metals is an operation of the greatest delicacy, and one in which a small error may be committed by the most skilful experimenter: besides, the atomic diameter =  $\sqrt[3]{\frac{\text{atomic weight}}{\text{specific gravity}}}$  and in the calculations, until all the primary laws are fully developed, we have to suppose the particles of all solids to be similarly situated, which certainly is not the case, and in solids, the error may amount to one-fourth the gravity; but since the metals are all fusible, and with the exception of few at a moderate temperature, compared with the total scale of heat which can be produced, the error will not be so great. If the table be extended to silex and other highly infusible substances, the general law is very apparent.

Upon the same principles many other properties of bodies may be investigated and phenomena explained; for example, if a heated body be coated with different substances, the layer being so thin as to produce no sensible effect by its conducting power, the radiating power will be inversely as  $F$ ; for the caloric is retained by the force of the surface only, and this power has been shown to be greatest in the most highly electro-negative bodies, or those which have the greatest tendency to the earth; and to this power that of radiation is inversely proportional: this accords very well with experiment; a heated clean metallic surface has a radiating power of 12; covered with a thin coat of glue, a highly positive body, it is 80; coated with lamp-black 100. The reflecting powers of polished metals appear to depend upon the density of the caloric contained in them, i. e. to the capacity (or rather the specific heat, if it were known) of equal volumes. The capacities of equal volumes are, iron 1.00, brass .97, silver .84, tin .51, lead .45, which numbers do not greatly differ from their powers of reflecting heat.

The conducting powers of solids for heat depend primarily upon the attraction and the capacity for heat; this power is nearly as the force  $F \times$  capacity of equal volumes, if equal lengths be used; by computation the powers are, gold 75,

silver 43, copper 38, iron 34, lead 24, zinc 23, tin 17, carbon about 39; but in order to compute correctly the conducting power of bodies, their radiating power must be experimentally ascertained; because it enters into the calculation. The law according to which caloric is conducted is easily determined; it is this:—If a solid rod be heated at one end, and distances be taken in arithmetical progression, the excess of temperature above that of the surrounding medium will decrease at those distances in geometrical progression.

The results of these investigations I consider as approximations whereby the general laws of chemical action are developed; and until these shall be correctly known, the corrections which the numbers require cannot be made. By electric energy, I mean no more than the fact, that bodies have definite tendencies to the poles of the galvanic series. If the particles of all bodies possess definite electrical states, the relation of the force of gravitation to electrical energy is clear; but this would give rise to results which are inconsistent with the known principles of philosophy: for it would follow that no solid can be simple, which may be the case; but since such hypothesis is totally unsupported by any evidence whatever, it cannot be admitted: also two masses of the same matter would be incapable of attracting each other, the contrary of which is fully proved by the experiments of Mr. Cavendish, and more decisive evidence cannot be desired: besides, if two bodies, A and B, both positive, attract a negative body, C, the force of A being greater than that of B; A and B will also attract each other, and the force will be proportional to the difference of their electric state; whereas in all cases the force is proportional to the quantities of matter: these phenomena militate against the hypothesis of the existence of electric energies. But if we suppose such a relation to exist between gravitation and electricity, that those bodies which have the greatest tendency to the earth are most powerfully attracted by the positive pole, all the phenomena admit of perfectly easy solution, and by electric energy nothing more will be meant than the relative tendencies of bodies to the poles of the battery, which is the sense in which I have used the term; and upon this hypothesis, chemical attraction, as well as cohesion, capillary attraction, adhesion, and gravitation, will depend upon, and be determined by the quantity of matter, to which there is evidence, that the phenomena of electric action may be reduced.

I remain, Gentlemen, yours, &c.

J. B. EMMETT.

## ARTICLE V.

*An Account of some Tumuli near the Falls of Niagara.*

By Major A. F. Macintosh.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

ABOUT three miles from the falls of Niagara, near the house of Sir P. Maitland, there is a ridge of rising ground, which commands an extensive view of Lake Ontario and the surrounding country, which is for the most part in this vicinity covered with wood.

On the most elevated part of this ridge, which is now called Mount Dorchester, about two years ago, a large oak tree, measuring at the base five feet in circumference, was blown down, and an opening made in the soil by the roots of the tree being torn from the earth, which exposed to view a quantity of human bones. The person who discovered that the accident had happened caused an excavation of about ten feet in diameter to be made, and found a deep stratum of human bones regularly disposed, and forming a vast number of perfect skeletons. The wrist bones of many of the skeletons had a species of armlets upon them; the head of a tomahawk, several Indian pipes, beads, and other ornaments, were also found interred amongst the skeletons; and the conjecture suggested by the discovery is, that the remains in question are those of some of the aboriginal inhabitants of the country who had fallen in some sanguinary conflict on this spot, and found their graves upon the field of battle.

The most interesting part of the discovery, however, consists in the circumstance of many large conch shells, some of them bored so as to be used as a rude kind of musical instrument, having been found disposed under the heads of several of the skeletons. Several fragments of the shells were also found near the upper parts of the bodies, and seem to have been worn upon the shoulders and arms, either as armour, or for the purpose of ornament, as they are perforated with holes, which probably were intended to put fastenings into to secure them upon the person. I was assured that these shells were of a species which is only found on the western coast of America, and on the shores of the neighbouring islands within the tropics. On seeing the shells, I immediately recollected that in the Museum at New York, there is a dress which belonged to the son of the King of Owhehee, which was brought to Europe originally by one of Capt. Cook's vessels, and that upon the same dress there is a conch shell of the same species of the

Niagara ones, which forms a very conspicuous ornament. Does not an investigation of this subject promise to throw some light on the history of the original population of the American continents, and the islands of the Pacific?

The spot where these remains were found bears every appearance of having been an Indian encampment. The ground on the side of the Lake, which is distant about seven miles, seems to have been rendered steep by artificial means; and Mr. Rorbach, who first discovered the bones, says, that when the ground is freed from the leaves of trees, which are every where strewed over it in great thickness, that holes resembling the marks of pickets may be seen surrounding a space of several acres. We should hence infer, that those warriors who fought with the tomahawk, and who used shells as musical instruments, and as defensive armour, were not ignorant of the art of war, so far as the construction of an extensive encampment defended by works possessing some pretensions to regular fortification, goes.

Where the first excavation was made, there can be little doubt that a tumulus had originally been constructed over the bones, as within a short distance of the first opening, four heaps resembling tumuli have been opened, and found to contain bones and ornaments of the kind which I have described.

The people in the neighbourhood have carried away many of the skulls, particularly the entire ones. I, however, succeeded, with the assistance of Mr. Rorbach, in collecting some of the most perfect of the remains, and took measures to insure their reaching Europe in safety, intending them for a scientific friend, from whose knowledge on such subjects, it may be hoped, that interesting results are to be looked for, should he be afforded the opportunity of examining these relics of an ancient and obscure period.

From the side of the hill rises a fountain of the most transparent water, in quantities sufficient to turn the wheel of a mill which is situated at a short distance; this is the invariable attendant of such tumuli, whether they occur in Britain, Scandinavia, or in Asia; and I could not help regretting that the tumuli of Niagara had not been inspected by some of those literary characters who have exhibited so much learning, and brought to light so much interesting and curious knowledge in their treatises upon the barrows and tumuli of Europe, Asia, and Africa, as undoubtedly those at Niagara, when taken together, with the remains of a similar character, which Baron Humboldt describes as existing in Mexico, might be the means of throwing light upon a period of the history of the world, where records entirely fail us, and which seems buried in the darkness of the most remote antiquity.

*List of the Articles sent to England by Major Macintosh.*

A skull, and three thigh bones.  
 A brass kettle.  
 A sheet of metal.  
 Several strings of coloured glass beads.  
 Some strings of beads, apparently made of shells and bones.  
 The head of a pipe.  
 A conch shell entire.  
 Several pieces of the same kind of shell shaped into ornaments.

ARTICLE VI.

*On Fluoric Acid, and its most remarkable Combinations.*

By Jac. Berzelius.

(Continued from vol. viii. p. 457.)

*Silicated Fluato of Potash.*—When this salt is precipitated from a weak acid, the liquid does not immediately become turbid, but the salt which exists diffused through it in very minute particles, communicates to it the property of reflecting the prismatic colours: by degrees these subside and form a transparent layer, which still exhibits a similar play of colours. While moist, this salt presents the appearance of a gelatinous mass, but is converted into a fine, soft, white powder by desiccation. It is very difficultly soluble in water, but not so much so that it can in every case be employed advantageously in making a quantitative determination of potash. It is rather more soluble in boiling than in cold water, and if a saturated solution be evaporated, the salt may be obtained in small crystals, which are sometimes rhombs, and sometimes regular six-sided prisms. The crystals are anhydrous. In a low red heat it melts, and if the temperature be augmented, it boils and gives off fluato of silica, but a very high temperature is necessary to produce complete decomposition. In the open air, fluato of silica is disengaged before the salt begins to undergo fusion. If the ignition be performed in an open platinum crucible, particularly if the heat of a spirit lamp be employed, a portion of the fluato is decomposed at the instant of its disengagement by the circumambient vapour of water, and the neutral fluato which remains at the conclusion of the decomposition is found to be mixed with silica. Hence, when I wished to ascertain the weight of the residual salt, I always placed the platinum crucible containing the silicated fluato within two others, and heated it in a charcoal fire: in these experiments, the interior of the first

or outermost crucible, and even of the second, was uniformly coated thickly with silica.

This salt is not altered by a solution of potash or of carbonate of potash in the ordinary temperatures, but if the mixture be boiled, carbonic acid gas is disengaged, and the whole of the salt passes into solution. In a boiling temperature, the liquid may be concentrated without any deposition ensuing. These effects, however, are not the result of mere solution; for the salt is decomposed, and the silica gelatinizes in proportion as the liquid cools. Gay-Lussac and Thenard have stated, that a subsalt consisting of silica, potash, and fluoric acid, may be formed by treating the ordinary silicated fluate with caustic potash; but the precipitate obtained in this manner is nothing else than a mixture of silica with the undecomposed salt.

*Silicated Fluate of Soda.*—This salt, whose existence has been denied by Gay-Lussac and Thenard, is almost identical, both in its general appearance and in its chemical characters, with the silicated fluate of potash. It is, however, heavier, and forms larger granules, on which account it subsides more rapidly in the liquid from which it is precipitated; and I have never observed it, when in this state, reflecting the prismatic colours. It has a gelatinous appearance while moist, but is converted into a fine mealy powder by desiccation. It is much more soluble in water than the salt of potash: it is also more soluble in boiling than in cold water, and its solubility is not increased by the presence of an excess of acid. When a saturated solution of the salt is evaporated in a moderate heat, it shoots in small shining crystals, which appear to be regular six-sided prisms, with transversely truncated extremities. The crystals contain no chemically combined water. This salt is acted upon by heat in a similar manner with the preceding, only it retains its excess of acid with much less obstinacy. The introduction of some bits of carbonate of ammonia into the crucible facilitates the dissipation of the last portions of this excess, but in this case the neutral salt which remains is always mixed with silica.

*Silicated fluate of lithia* is almost insoluble in water. Its solubility is augmented by an excess of acid, and it may be obtained by this means in small transparent crystals, which are occasionally six-sided prisms, but which have evidently a rhomboid for their basis. When heated, it melts, and obstinately retains its fluate of silica.

*Silicated Fluate of Ammonia.*—This salt may be formed in the humid way by saturating the liquid acid with ammonia, but the operation is attended with difficulty, because the alkali, even when very dilute, has the property of decomposing the fluate of silica. In the dry way it may be easily prepared by distilling a mixture of the silicated fluate of potash or soda with sal ammoniac. Thus obtained, it constitutes an uncrystalline mass, but

if it be dissolved in water, and the solution committed to spontaneous evaporation, it shoots in large transparent crystals. The primary form of its crystal is the rhomboid, and, like the preceding salts, it has a strong tendency to assume the form of a short six-sided prism. This salt is very soluble in water. Ignited, it decrepitates slightly, and sublimes unaltered; and a glass retort may be employed for this experiment without undergoing corrosion. Ammonia decomposes the aqueous solution of this salt, but if the filtered liquid (which still retains some silica in solution) be evaporated, a certain quantity of the alkali is volatilized, and a portion of the double salt is regenerated, in consequence of the silica being redissolved by the disengaged acid.

Gay-Lussac and J. Davy have shown that the gaseous fluato of silica and ammoniacal gas occasion mutual condensation, when mixed in the proportion of two volumes of the former to one volume of the latter. The product is a white pulverulent salt, which may be sublimed unaltered, so long as it is kept free from moisture. When put into water, it is decomposed, and the silica evaporates in a gelatinous state, according to J. Davy;—a proof that it had been chemically combined with the other ingredients of the salt. This compound appears, therefore, to consist of an atom of anhydrous fluato of ammonia and an atom of anhydrous silicate of ammonia; and it probably belongs to the class of salts styled fluosilicates.

*Silicated fluato of barytes* is best obtained by mixing a solution of muriate of barytes with the liquid acid: after a few moments it precipitates in minute crystals, and the liquid contains disengaged muriatic acid. It is so little soluble in water that nearly the whole of the barytes may be in this manner precipitated, and its solution is not sensibly promoted by the excess of muriatic acid. Its crystals are prisms, with very long acuminations. It contains no water of crystallization. When heated, it is easily decomposed, and there remains neutral fluato of barytes.

*Silicated fluato of lime* may even be obtained by digesting a mixture of pulverised fluor spar and silica in muriatic acid; but the most certain method of preparing it is to add carbonate of lime to the liquid acid so long as it continues to dissolve. This salt is insoluble in water, unless when assisted by an excess of acid, and it crystallizes as this excess evaporates. The crystals, which are well characterized, appear to be four-sided prisms with obliquely truncated terminations. When digested in water, this salt is partially decomposed; fluato of lime and silica being precipitated, while the liquid silicated fluoric acid which is in this manner disengaged, retains the remainder of the double salt in solution.

*Silicated fluato of strontian* is easily soluble in water, containing an excess of acid, and may be obtained in large crystals by evaporation. These crystals are short four-sided slightly oblique

prisms, and have a two-sided acuminat<sup>ion</sup> which rests upon the opposite acute angles of the prism. They contain water of crystallization, and become enamel white and opaque when heated. Water decomposes this salt, but to a much less extent than the preceding. The difference between the properties of the salts of barytes and strontian furnishes an easy and exact process, both for distinguishing these two earths from one another, and for separating them when in a state of mixture. For this purpose, a solution of the earths in muriatic or acetic acid is to be mixed with liquid silicated fluoric acid, and the amount of the barytes is to be determined from the weight of the precipitated double salt. A very small quantity of sulphuric acid precipitates the barytes which remains in solution without acting upon the strontian, and by evaporating the filtered liquid to dryness, and decomposing the residue by sulphuric acid, the latter earth may be obtained in the state of sulphate.

*Silicated Fluat<sup>e</sup> of Magnesia.*—A transparent, yellowish, gummy looking mass, easily soluble in water.

*Silicated Fluat<sup>e</sup> of Alumina.*—A clear colourless jelly, which, when dried, splits into fragments, and appears yellowish, but still retains its transparency. It dissolves slowly but completely in water.

*Silicated fluat<sup>e</sup> of glucina* is readily soluble in water, and is converted by evaporation into a colourless syrup, which finally becomes white and opaque. Its taste is astringent, without any admixture of sweetness.

*Silicated fluat<sup>e</sup> of yttria* is insoluble in water, but dissolves in an excess of acid.

*Silicated fluat<sup>e</sup> of zirconia* dissolves very easily in water, and may be obtained by evaporation in white crystals, which have the lustre of mother-of-pearl. The solution becomes opaque when boiled, but the greater part of the salt continues dissolved.

*Silicated fluat<sup>e</sup> of oxide of zinc* is obtained by dissolving zinc in the liquid acid. It is extremely soluble in water, and is deposited from a concentrated solution in crystals which are generally equiangular three-sided prisms. The crystals are not altered by exposure to the air.

*Silicated fluat<sup>e</sup> of oxidule of manganese* is very soluble in water, and crystallizes on cooling from a concentrated solution in long thin regular six-sided prisms. Sometimes it is obtained by spontaneous evaporation in very short six-sided prisms, which distinctly indicate the rhomboid as their basis. The crystals have a just perceptible tinge of amethyst red. It is converted by ignition into the simple fluat<sup>e</sup> without losing its crystalline form.

*Silicated Fluat<sup>e</sup> of Oxidule of Iron.*—When a solution of this salt, prepared by dissolving iron filings in the liquid acid, is allowed to evaporate in a capsule of metallic iron, it shoots in

bluish green coloured regular six-sided prisms; but the liquid is converted into a dry mass so soon after it begins to crystallize, that, unless we operate upon large quantities, it is difficult to obtain the salt in perfect crystals. A second crystallization renders the salt paler coloured and more regularly formed. I have remarked that all the coloured salts belonging to this class have a deeper colour than usual when crystallized from a solution containing an excess of acid; but this difference in appearance does not seem to be accompanied by a corresponding difference in their composition.

*Silicated Fluato of Oxide of Iron.*—A semitransparent, pale flesh coloured mass. It dissolves in water, and the solution is faintly coloured.

*Silicated fluato of oxide of cobalt and of oxide of nickel* are easily soluble in water, and crystallize in forms which are exactly similar to those of the salts of manganese and iron. The crystals are rhomboids, but pass into regular six-sided prisms, whenever they are in a situation to elongate themselves. The salt of cobalt is red; that of nickel green.

*Silicated fluato of oxide of copper* is easily soluble in water, and shoots by spontaneous evaporation in transparent blue coloured crystals, which are more determinately rhomboidal than the preceding, but which have still a decided tendency to become six-sided prisms. The crystals effloresce externally and become opaque when exposed to the air, and their colour at the same time changes to a light blue.

The remarkable coincidence between the crystalline forms of the greater number of the salts formed by the preceding isomorphous metallic oxides, led me to suspect that they might all contain a similar number of atoms of water of crystallization. I examined, therefore, the salts of oxidule of manganese and of the oxides of zinc, cobalt, nickel, and copper, and found that they all contain a quantity of water of crystallization whose oxygen is seven times that of the base. The fatiscerated salt of oxide of copper still retains a quantity of water whose oxygen is five times that of the oxide of copper.

*Silicated fluato of oxidule of copper* has a red colour, and closely resembles the corresponding simple fluato both in external appearance, and in the decomposition which it sustains through the combined action of air and moisture. In a high temperature it melts, and loses its fluato of silica.

*Silicated Fluato of Oxide of Lead.*—A transparent gummy-looking mass, soluble in water, and possessing the peculiar taste of the salts of lead.

*Silicated fluato of oxide of cadmium* is extremely soluble in water, and crystallizes in long colourless prisms, which contain water of crystallization.

*Silicated fluato of oxidule of tin*, like the preceding, is very

soluble in water, and crystallizes in long prisms; but it is partially oxidized, and decomposed during evaporation; and the oxide thus formed precipitates in the state of a silicate.

*Silicated Fluato of Oxidule of Chromium.*—A green coloured uncrystallizable transparent mass, which deliquesces to a liquid when exposed to the air.

*Silicated fluato of oxide of antimony* is easily soluble in water containing an excess of acid. By slow evaporation it crystallizes in prisms, which, after being dried, rapidly fall to powder.

*Silicated fluato of oxidule of mercury* may be prepared by digesting newly prepared and still moist oxidule in the liquid acid. It is by this means converted into a pale straw yellow coloured powder. The liquid, particularly when it contains an excess of acid, retains a portion of the salt in solution, which it deposits in small crystals when evaporated. The solution of this salt has a weak metallic taste, and is copiously precipitated by muriatic acid.

*Silicated fluato of oxide of mercury* is soluble only in an excess of acid, and crystallizes by evaporation in small yellowish coloured or almost colourless needles. When put into water, this salt is partly converted into a yellow coloured insoluble subsalt, while the remaining portion is held in solution by the disengaged acid. When ignited, gaseous fluato of silica is in the first place expelled, and the fluato which remains undergoes decomposition in the manner already described. The yellow insoluble subsalt is blackened by ammonia; but its colour is again rendered lighter by the addition of water.

*Silicated fluato of oxide of silver* is a very deliquescent salt, which may be obtained in white granular crystals from a solution concentrated to the consistence of a syrup. A small quantity of ammonia precipitates from the solution a light yellow coloured subsalt, which, when added in excess, it redissolves, and leaves a silicate of oxide of silver.

*Silicated Fluato of Oxide of Platinum.*—A yellowish brown coloured salt, very soluble in water. When evaporated to a tenacious syrup, and in this state digested in water, it leaves a brown coloured subsalt undissolved.

*Fluosilicates.*—I shall hereafter discuss the different points of view under which both the foregoing series of compounds, and those which still remain to be described, may be regarded. At present I shall merely add, that however much we may at first feel disposed to do so, the silica cannot in these compounds be considered to act as an acid but as a base, and consequently that the name of *silicate* when applied to them implies an idea which their nature does not authorise. The mineral kingdom, however, furnishes us with examples of compounds in which a fluato is actually associated with a silicate, and for which therefore the appellation of fluosilicate would be sufficiently appro-

priate. Thus the *topaz* consists of an atom of subfluat of alumina combined with nine atoms of silicate of alumina; and *pycnite*, of an atom of the neutral fluat combined with nine atoms of the silicate.

During the decomposition of the silicated fluates by the caustic alkalies, particularly by ammonia, it is possible that other fluosilicates may be produced, in which the relative proportions of the fluat and silicate may vary with the different circumstances under which the compounds are formed. I have not investigated this subject so minutely as it deserves, and indeed I have confined myself to the decomposition of the silicated fluat of lime by ammonia, as being that of which an accurate knowledge is at present most interesting, because the precipitates which result from this decomposition occasionally make their appearance during the analysis of minerals. A mixture of finely pulverized fluor spar and of ignited silica in the state in which it is obtained from the decomposition of the fluat of silica, was digested with muriatic acid in a closely stopped glass vessel, from which no vapours of fluat of silica could escape. At the end of 48 hours, the clear liquid was mixed with ammonia, and the precipitate was washed and ignited. Decomposed by sulphuric acid, this precipitate gave off gaseous fluat of silica, which was received in carbonate of soda, and left 136 per cent. of sulphate of lime. The alkaline solution was evaporated to dryness in a moderate heat; and the residue, being digested in water, left 22.11 per cent. of silica. The remaining liquid was saturated with acetic acid, exposed to the air for 24 hours, in order to ensure the dissipation of the carbonic acid, mixed with ammonia, and precipitated in a stoppered vessel with muriate of lime. The fluat of lime thus obtained weighed, after ignition, 78 per cent. The precipitate was composed, therefore, of neutral fluat of lime and of silica in the proportions requisite to form with fluoric acid the liquid silicated fluoric acid. Whether the silica actually existed in a state of chemical union is doubtful, but it appears to be rendered probable by the fact, that the neutral alkaline fluates are capable of dissolving silica in a red heat without undergoing decomposition.

Another portion of the same solution in muriatic acid was mixed with muriate of lime, and decomposed by ammonia. The precipitate, analyzed in the same manner as the preceding, yielded 150 per cent. of sulphate of lime = 62.25 per cent. of lime, 19 per cent. of silica, and 65.67 per cent. of fluat of lime = 18.04 of fluoric acid. It appears, therefore, to have been composed of an atom of bisilicate and three atoms of fluat of lime. The precipitate formed by ammonia in a solution of apophyllite in cold nitric or muriatic acid, and which many chemists have mistaken for alumina, possesses an exactly similar composition. If the mineral be dissolved with the assistance of heat.

silicated fluoric acid is volatilized; neither do we obtain the compound by evaporating the acid solution to dryness, because when a solution of fluor spar and silica in an excess of muriatic acid is evaporated, there remains nothing except muriate of lime. The double silicated salts of those bases from which ammonia separates a portion of their fluoric acid would probably give precipitates with that alkali, in which a different relation would exist between the proportions of the silicate and fluuate.

(To be continued.)

## ARTICLE VII.

*Astronomical Observations, 1824 and 1825.*

By Col. Beaufoy, FRS.

*Bushey Heath, near Stanmore.*

Latitude  $51^{\circ} 37' 44.3''$  North. Longitude West in time  $1^{\text{h}} 20.93''$ .

1824.

Dec. 16.	Immersion of Jupiter's first	{ 10 <sup>h</sup> 0' 25"	Mean Time at Bushey.
	satellite .....	{ 10 01 46	Mean Time at Greenwich.

1825.

Jan. 4.	Immersion of Jupiter's third	{ 12 37 01	Mean Time at Bushey.
	satellite .....	{ 12 38 22	Mean Time at Greenwich.
Jan. 8.	Immersion of Jupiter's first	{ 10 09 16	Mean Time at Bushey.
	satellite .....	{ 10 10 37	Mean Time at Greenwich.
Jan. 11.	Immersion of Jupiter's third	{ 16 35 19	Mean Time at Bushey.
	satellite .....	{ 16 36 40	Mean Time at Greenwich.

Occultation by the Moon.

Dec. 31.	Immersion of $\xi$ Pisces. ....	6 <sup>h</sup> 26' 46"	Siderial Time.
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Francis Baily, Esq. having favoured me with the new method of determining the longitude by the culmination of the moon and stars; together with a list of stars applicable to the purpose for the year 1825, the following observations were made at Bushey Heath:—

Transit over the Middle Wire in Siderial Time.

Jan. 4.	{	$\xi$ Gemini. ....	6 <sup>h</sup> 53	46.46"
		Moon's First Limb. ....	7 01	08.95
		$\alpha$ Gemini. ....	7 11	40.15

## ARTICLE VIII.

*On a peculiar Class of Combinations.* By Dr. F. Wöhler.\*

WITH the intention of preparing cyanuret of silver by the reciprocal decomposition of cyanuret of mercury and nitrate of

\* Annalen der Physik.

oxide of silver, I mixed pretty concentrated solutions of the two compounds: contrary to my expectation, no precipitate fell; but after a few minutes there was deposited a number of small white crystals, whose quantity greatly exceeded that of the cyanuret of mercury which I had employed. They were repeatedly washed with water, and dried.

When these crystals are heated in a temperature above  $212^{\circ}$ , they fuse in the first place into a transparent liquid; by and by they boil up and detonate vehemently, with a crackling noise, and a purplish red coloured flame, closely resembling that which accompanies the combustion of cyanogen. The residue consists of cyanuret of silver, and, by continued ignition in the open air, is converted into metallic silver. If the experiment be performed in a glass tube, a quantity of mercury is also sublimed. Muriatic acid, poured upon the crystals, instantly disengages hydrocyanic acid, and after the whole of the latter has been expelled by the application of heat, there is given off a strong odour of chlorine: the liquid, evaporated to dryness, leaves a mixture of the chlorides of silver and mercury. If a solution of the crystals be precipitated by muriate of barytes, and if the filtered liquid be evaporated, there is obtained a saline mass, containing abundance of octohedral crystals of nitrate of barytes. From the saline mass alcohol extracts cyanuret of mercury. Consequently this crystallized substance is a compound of cyanuret of mercury and nitrate of oxide of silver.

This compound is very difficultly soluble in cold, but rather copiously in hot water, and as the solution cools, it crystallizes in large transparent prisms, having the form of saltpetre. It may be obtained in large crystals also by mixing hot solutions of the cyanuret of mercury and nitrate of silver; the crystals appearing as the liquid cools. Alcohol appears to dissolve it in nearly the same proportions as water. In boiling hot nitric acid it is soluble without decomposition. Alkalies precipitate from its aqueous solution cyanuret of silver, which appears to be mixed with subnitrate of oxide of mercury. Repeated solutions in pure water produce a similar decomposition; but only to a very inconsiderable extent.

When these crystals are heated in a temperature rather below  $212^{\circ}$ , they give off water, and become white coloured and opaque, without losing their original form. 100 parts, thus treated, lost 7.6 parts of water.

To determine the quantity of silver, 1 gramme of the crystals was treated with an excess of muriatic acid, and the mixture was cautiously evaporated to dryness. The corrosive sublimate being now expelled from the dry mass by ignition, there remained 0.32 gramme of fused chloride of silver. This is equivalent to 0.2588 gramme of oxide of silver, and consequently indicates 37.96 per cent. of nitrate of oxide of silver. The quantity of

cyanuret of mercury was ascertained by dissolving 0·67 gramme of the crystals in hot water, and precipitating the silver by cyanic acid. The filtered liquid was then evaporated to dryness, in order to expel the excess of cyanic acid, and the disengaged nitric acid. 0·36 gramme of pure cyanuret of mercury remained = 53·74 per cent.

Hence 100 parts of this compound consist of

Nitrate of oxide of silver . . . .	37·96	.....	1 atom	
Cyanuret of mercury . . . . .	53·74	.....	2	
Water . . . . .	7·60	.....	8*	,
	99·30			

Here therefore we have a compound destitute of oxygen, and analogous to the metallic sulphurets and chlorides, associated in determinate proportions with another compound, which belongs in the strictest sense of the word to the class of salts. As we know that many bodies exert sometimes an electro-positive and at other times an electro-negative action, and that many compounds, which, by themselves, appear of an indifferent nature, may assume either of these characters with reference to certain other substances, it follows, that the compound here examined must, in this point of view, be regarded as a saline combination, in which the nitrate of oxide of silver acts as the acid, and the cyanuret of mercury as the base. The existence of water of crystallization in the compound, which neither of its ingredients in a separate state possesses, affords an additional argument for ranking it in the class of salts. Berzelius, when he formed the white crystalline compound of prussian blue and sulphuric acid, was the first person who discovered the existence of this class of combinations.

I now attempted to form other compounds, in which the nitrate of oxide of silver would act as an acid when united with metallic cyanurets.

Newly precipitated and washed cyanuret of silver was boiled in a solution of nitrate of silver: it dissolved slowly, but completely. As soon as the temperature fell a few degrees below the boiling point, there was deposited a large quantity of long white shining needles, so that the liquid became converted almost into a magma. They were transferred upon blotting paper and dried. This compound cannot be washed, for the affinities by which it is maintained are so feeble, that when placed in contact with water, it is instantly resolved into pulverulent cyanuret of silver, and the soluble nitrate. Hence in its preparation it is necessary to employ a pretty concentrated

\* Or 4 atoms of water, adopting Dr. Thomson's numbers.—*Ed.*

solution of nitrate of silver. When heated, this compound fuses, then detonates with considerable energy, and leaves cyanuret of silver, which probably contains a minimum of cyanogen. It contains no water. If its constitution be analogous with that of the foregoing salt, it ought to be composed of

Nitrate of oxide of silver . . . . .	1 atom . . . . .	38.79
Cyanuret of silver . . . . .	2 . . . . .	61.21
		100.00

It ought, therefore, to contain 70.76 per cent. of metallic silver. This was confirmed by an experiment in which 0.43 gramme of the salt, decomposed by muriatic acid, yielded me 0.387 gramme of fused chloride of silver, equivalent to 69.74 per cent of metallic silver.

I made many attempts, but without success, to form analogous compounds by boiling other metallic cyanurets in a solution of nitrate of silver. Cyanuret of nickel, treated in this manner, instantly gave cyanuret of silver, and nitrate of oxide of nickel: a similar decomposition took place with cyanuret of zinc. Prussian blue occasioned the evolution of nitrous gas, and there was obtained a solution of nitrate of oxide of iron, and a precipitate consisting of a mixture of oxide of iron and cyanuret of silver. Cyanuret of lead yielded a solution of nitrate and subnitrate of lead, and a black coloured precipitate, which the application of nitric acid proved to consist of metallic silver and white cyanuret of silver. Cyanuret of copper, boiled in a solution of nitrate of silver, gave a precipitate consisting entirely of metallic silver. Cyanuret of palladium, similarly treated, sustained no alteration.

## ARTICLE IX.

*A List and Description of some Species of Shells not taken Notice of by Lamarck.* By John Edward Gray, Esq. MGS.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

*British Museum, Jan. 10, 1825.*

IN the following list I have referred several species, which have not been taken notice of by Lamarck, to his genera, and have described some new ones that are contained in the collection in the British Museum, where most of the species are exhibited with the names, here adopted, attached.

Your obedient servant,

J. E. GRAY.

## 1. MOLLUSCA CONCHIFERA.

*ASPERGILLUM Javanum*, Lam. disco subperforato, tubuli fimbriæ distinctis crassis, *Martini*, t. 1, f. 7.

*A. Listeri*. disco confertissime perforato, tubulis fimbriæ confertis tenuibus, *List.* t. 548, f. 3. *A. vaginitærum*, *Lam.*? I think that all the species of this genus will be found to have a foliaceous mouth to their tube when they are perfect.

*Mya Binghami*. *Sphænia Binghami*, *Turton*.

*ANATINA*. The shells of this genus always have a loose piece in their hinge which is very much developed in *A. Norvegica*, but is distinctly to be found in *A. Prætenus* and *A. Myalis*.

*Anatina globosa*. *Mya globosa*, *Wood*, t. 24, f. 4—6.

*An. Nicobarica*. *Mya Nicobarica*, *Gmelin*.

*An. prætenus*. *Mya prætenus*, *Montague*, t. 1, f. 2.

*An. distorta*. *Mya distorta*, *Montague*, t. 1, f. 1.

*An. convexa*. *Mya convexa*, *Wood*, t. 18, f. 1.

*An. Norvegica*. *Mya Norvegica*, *Chemn.* x. 1647, 1648. *Amphidesma corbuloides*, *Lam. Hist.* 492.

*An. membranacea*. *Mya membranacea*, *Dillwyn*, 48.

*LUTRARIA vitrea*. *Mactra vitrea*, *Chemn.* xi. f. 1959, 1960.

*L. fragilis*. *Mactra fragilis*, *Chemn.* vi. f. 235.

*MACRA Campechensis*. *List.* 304, f. 141.

*M. squamosa*. *Solen squamosus*, *Montague*.

*ERYCINA*. *Lam.* Several of Lamarck's *Crassatellæ* agree with the character of this genus; therefore I have removed them as far as I have any grounds. The recent species of Lamarck is a *Cytherea*.

*Ery. denticulata*. Testa elongato-cuneata, dentibus lateralibus serrulatis.

*Ery. striata*. *Crassatella striata*, *Lam.* 483.

*Ery. subangulata*. *Crassatella cuneata*, *Lam.* 483!

*Ery. glabrata*. *Crassatella glabrata*, *Lam.* 482.

*Ery. ovata*. Testa ovato-elongata, cardine in medio recto.

*Ery. Australis*. *Mya Novæ Zealandiæ*, *Chemn.* vi. f. 19, 20.

*UNGULINA*. The only species of this genus that I have seen appear to be too nearly allied to *Amphidesma* to be kept distinct.

*AMPHIDESMA decussatum*. *Tellina decussata*, *Wood*, t. 43, f. 2, 3.

*Amph. cordiforme*. *Tellina cordiformis*, *Chemn.* xi. f. 19, 41, 42.

*Amph. variabile*. *Tellina obliqua*, *Wood*, t. 41, f. 4, 5.

*Amph.? nitens*. *Mya nitens*, *Montague*.

*CORBULA labiata*. *Mya labiata*, *Maton*, *Lin. Trans.*

*PANDORA glacialis*. Testa semicircularis, cardine submedio, margine dorsali recto.

**LITHOPHAGÆ.** The whole of the genera of this family appear to have very great affinity to the *Cardita*, *Cypricardiæ*, &c. and should be placed nearer to them in a natural arrangement as well as the latter genera themselves; but these genera appear to be the most defective part of Lamarck's arrangement.

*PETRICOLA costata*, *Lam. Syst.* Venus *Lapicida*, *Chemn.* x. f. 1665, 1666.

*Pet. divergens.* Venus *divergens*, *Gmelin.*

*Pet. nivea.* *Mytilus niveus*, *Chemn.* viii. t. 82, f. 734.

*Pet. suborbicularis.* *Mya suborbicularis*, *Montague.*

*Pet. bidentata.* *Mya bidentata*, *Montague.*

*Pet. rubra.* *Cardium rubrum*, *Montague.*

*VENERUPIS monstrosa.* Venus *monstrosa*, *Chemn.* vii. f. 42.

*Ven. decussata.* *Mya decussata*, *Montague.*

*TELLINA tenera.* *Macroma tenera*, *Leach.*

*LUCINA Childrenæ.* Testa suborbiculata inequivalvis alba subantiquata; tenuissime radiata substriata: long. 3 unc. Brazil, *Humphreys*, nob. *Zool. Jour.* i. 221.

*Luc. gibba.* *Tellina divaricata* var. *Chemn.* vi. f. 130.

*Luc. globosa.* Venus *globosa*, *Chemn.* vii. f. 430, 431.

*Luc. scabra.* *Tellina scabra*, *Chemn.* xi. f. 1943, 1944.

*Luc. divaricata. var.?* *Tellina dentata*, *Wood*, t. 46, f. 6.

*TELLENIDES?* *triangularis.* *Tellina triangularis*, *Chemn.* vi. t. 10, f. 85.

*DONAX veneroidea.* Venus *donaci formis*, *Chemn.* xi. f. 1983, 1984.

*Don. scalpellum.* Testa elongata, complanata, tenuis purpureo radiata, polita, tenuissime radiato-striata; antice valde elongata rotundata, lutea; postice oblique truncata, biangulata, purpurea, margine minute denticulato.

*CRASSINA borealis.* Venus *borealis*, *Chemn.* vii. f. 412—414. *Cyrena! depressa*, *Lam.?*

*Crass. triangularis.* *Mactra triangularis*, *Montague.*

*Crass. minutissima.* *Mactra minutissima*, *Montague*, An var. *prioris?*

*Crass. minima.* Venus *minima*, *Montague*, t. 3, f. 3.

*Crass. subcordata.* Venus *subcordata*, *Montague*, t. 3, f. 1.

*Crass. sulcata.* Venus *sulcata*, *Montague*, *Lam.* 427.

*Crass. Montagu.* Venus *compressa*, *Montague*, t. 26, f. 1.

*Crass. Scotica.* Venus *Scotica*, *Maton*, *Lin. Trans.* t. 2, f. 3.

*Lap.* 155.

*Crass. Banksii.* *Nicania Banksii*, *Leach.*

*Crass. striata.* *Nicania striata*, *Leach.*

*CYRENA cyprinoides.* Testa cordato-trigona, gibba, olivacea, concentrice sulcata; cardine incrassata, dentibus lateralibus lævibus, anteriori conico cæteris approximato. Japan, long. 15-16, unc.

*Cyr. Childrenæ.* Testa orbiculato-cordata, lævis olivacea antice distanter, irregulariter concentrice costata, intus purpureo aurantia; dentibus lateralibus serrulatis. *Encyc. Method.* t. 301, f. 1, long. 2 unc. Lamarck has referred this figure to *Cyprina Islandica*, but the teeth are evidently serrulated, &c.

*Cyr. limosa.* Tellina limosa, *Maton, Lin. Trans.* x. t. 24, f. 8—10.

CYTHEREA (*b*) *albida.* Venus albida, *Gmelin, List.* 273, f. 109.

*Cyth. (a) crassa.* Testa cordato-triangulata, gibba, crassa, polita, lutea, latere postico purpureo livido, lunulâ lanceolato-cordata magnâ; dentibus valde incrassatis. long. 18-10, unc. *Madras, Humphreys, Mus Cracherode.*

*Cyth (a) pinguis.* Testa cordato-triangulata solida, polita lutea lurida; umbonibus biradiatis; latere postico lunulâque purpureo-livida; intus carneo-albidis, punctis fuscis ornatis. long. 13-18 unc. *Bombay, Humphreys, Mus. Cracherode.*

*β minor* subradiata striata, margine tumido.

*Cyth. (a) scripta.* Donax scripta, *Lin. Lam.!*

*Cyth. (a) Solanderii.* Testa ovata gibba, lævi polita albida purpureo variegata; umbonibus stellatis; intus albida; margine crenato; latere postico maculis purpureis natato. long. 13-18. "Venus hyans *Soland. MSS.*" *Humphreys.* Like the former, but much more gibbous, and in the different teeth none of these three species have any affinity to *Donax*, with which Lamarck placed them.

*Cyth (a) meroe.* Venus meroe, *Lin. Donax! meroe, Lam. Venus donaciformis, Gmelin.*

*Cyth. (b) cardoides.* Erycina cardoides, *Lam.*

*Cyth. (b) exilis.* Venus exilis, *Chemn.* vi. t. 34, f. 362, 363.

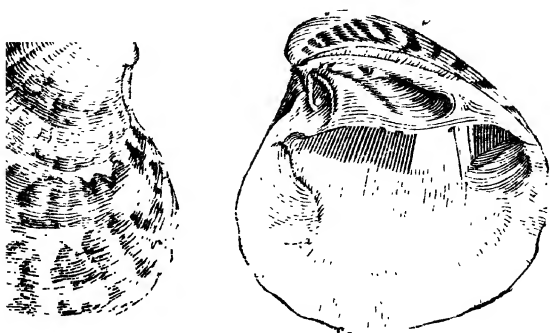
*Cyth. (d) Histrio.* Venus exoleta variegata, *Chemn.* vii. f. 407.

VENTS *aurisiaca.* Testa ovato-trigona, polita subconcentrice striata, pallide fusca, obscure trizonata; latere postico elongato; lunula scutulaque lanceolatis, purpureo variegatis: intus aurantiaca, long. 9-10 unc. *Mus Cracherode.*

*Ven? papyracea.* Testa ovata gibba papyracea tenui pellucida alba subantiquata; umbonibus concentrice sulcatis; margine cardinali antice impresso. An novum genus? An Lithophagæ? Testa peculiaris.

*Ven. rotundata.* Tellina rotundata, *Montague, t. 2, f. 3.*

VENERICARDIA *megastropa.* Testa oblique cordata crassa albida, rufo variegata, costis convexis rugosis; margine cardinali crassissimo. long. unc. *New Holland? E. dono. Dom. Bennet.* (See figure on next page.)



*CARDIUM semisulcatum*. Testa transversa, ovata lutea rosea vel albida, costato-striata, subspinoso; latere antico conferte striato; postico producto, aperto, distanter costato; margine dentato. long. 7-10 unc. *C. bullato* similis.

*Card. crenatum*. Testa cordata, alba, umbonibus carinatis, costis 22 convexis, anticis minoribus, lunula profundissima callosa intrusa.

*ARCA trigona*. Testa subcordata trigona turgida, angulata; latere antico plano. long. 1 unc. peculiar for having the form of *Hippopus maculatus*.

*NUCULA Montagni*. Arca rostrata, *Mont. Sup. t. 27, f. 7.*

*Nuc. minuta*. Arca minuta, *Muller.*

*Nuc. tenuis*. Arca tenuis, *Montague.*

*Nuc. glacialis*. Leubulus glacialis, *Leach.*

*UNIO ponderosa*. Mya crassa, *Wood, t. 20, 21.*

*Un. nodulosa*. Mya nodulosa, *Wood, t. 22, f. 1—4.*

*Un. plumbea*. Chama plumbea, *Chemn. xi. t. 203, f. 1991, 1992.* N. B. Chama is certainly the best Linnean genus for the freshwater bivalves with irregular teeth.

*HYRIA intermedia*. Testa ovato-subquadrata, virido-nigra lævis, antice rotundata, postice sinuata; umbonibus prominentibus. long. 26-8 unc. Inter *H. avicularem* et *H. elongatani*.

*Hyria Matoni*. Mya variabilis, *Maton, Lin. Trans. x. t. 24, f. 417.*

*ANODONTA* must be retained instead of *Anodon*, a change first proposed by Dr. Leach in this work, as the latter has been used for a genus of reptiles. If it must be altered, *monodonta*, and several others, will also require it.

*Anodonta fluviatilis*. Mya fluviatilis, *Dillw. 316, List, t. 157, f. 12.*

*Anodonta Adansonii*. Mytilus dubius, *Gmel. Adams. t. 17, f. 18.*

*BARBALA plicata*. Dipsas plicatus, *Leach, Zool. Misc.*

*Modiola castanea*. Testa convexa, subcylindrica, castanea pellucida, concentrice striata, *List*, t. 1065, f. 9, *Rumph.* t. 46, f. 2.

*Mod. Brasiliensis*. *Mytilus modiolus Brasiliensis*, *Chemn.* xi. f. 2018, 2019. *Mytilus latus jun*, *Dillw.*!

*MYTILUS dilatatus*. Testa trigona postice rotundata, compressa, umbonibus acutis incurvatis—Mediterranean.

*Myt? Volgensis*. *Mytilus fluvis*. Volga *Chemn.* *Myt.* polymorphus, *Gmelin*, perhaps will form a genus distinct from *Mytilus*, and peculiar for its freshwater habitation; and like shells of that station, the animal can live for a long time out of water. I have kept one for three weeks, when it was still healthy. It is found in the Commercial Docks, where it most likely has been introduced with timber from the Volga.

*CRENATULA*. This genus may be divided into two sections, which may perhaps hereafter be considered as genera by the same character as separates *Mytilus* from *Modiola*, § 1. *Testa quadrata umbonibus anterioribus*, which includes the species or rather varieties mentioned by Lamarck. § 2. *Testa ovata umbonibus sub anterioribus* (Dalacia) containing the following:

*Cre. folium*. Testa albida radiata compressa; latere antico rotundato, postico alata, Brande's Journal, xv. t. 2, f. 81. figura pulcherrima. *Vulsella folium*, *Humph.* *Mus* Cracherode.

*LIMA gigantea*. Testa crassa, ponderosa, subauriculata albidorozea, irregulariter radiata costata striata; intus alba, rufo maculata. lat. 15-4, long. 18-4, unc.

*Lim. excavata*. *Ostrea excavata*, *Gmelin*.

*OSTREA prismatica*. Testa elongata lamellosa; intus violacea, albido macerata iridescens; impressione muscularis reniformi translucens; umbonibus truncatis; valvâ superioris planulata. long. 2, lat. 6, unc.

*ANOMIA rosea*. *Tellina ænigmatica*, *Chemn.* x. t. 199, f. 1949, 1950. *Mus*. *Tankerville*.

*DISCINA*. This genus is certainly distinct from *Orbicula*, which appears to be the same as *Crania*.

*Dis. levis*. *Orbicula! lævis*, Sow.

## 2. MOLLUSCA PTEROPODA.

Lamarck, Cuvier, and Peron, appear to have reversed these animals and the *heteropes*, and called their belly their back, for they certainly, like the *gasteropodes*, swim with their belly upwards, and consequently the latter have their shell placed on their mantle as in the *gasteropodes*; to this order should be referred the genus *Janthina*.

## 3. MOLLUSCA GASTEROPODA.

*PLEUROBRANCHUS Montagui*. *Bulla Plumula*, *Montague*.

*Pleu. argenteus*. *Bulla membranacea*, *Montague*.

*Siphonaria angulata*. Testa convexo conica, angulata radiato-costata; intus fusca. long. 15-10 unc.

*PARMOPHORUS elegans*. Emarginula breviusculus, Sow. Gen. f. 2, certainly not *Parmophorus breviusculus* of Blainville, as that shell is in the Museum, and is only slightly antiquated. Inter *Parmophoros* et *Emarginulas*.

*EMARGINULA cristata*. Testa convexo-conica, antice costa media cristata ornata.

(To be continued.)

## ARTICLE X.

*An Account of a new Mineral.* By M. Lévy, MA. in the University of Paris.

(To Mr. Children.)

DEAR SIR,

THROUGH your kindness and that of Mr. James Sowerby, I have been enabled to examine some well-defined single crystals of a substance found at Snowdon, which had been classed by some with rutile, by others with sphene, but which certainly differs from both, its forms being derivable from a right rhombic prism, whilst the primitive form of rutile is a square prism, and that of sphene an oblique rhombic prism. The forms of this substance I have observed are represented by figs. 2, 3, and 4, and although I have not drawn the inferior summit, some of the planes which belong to it occur in some of the crystals. They are flattened parallel to the planes  $h^1$ , and some are more than half an inch in breadth and length. They cleave easily in a direction parallel to the plane  $g^1$ , but the face of cleavage is rather dull. All the natural planes are sufficiently brilliant to be

Fig. 2.

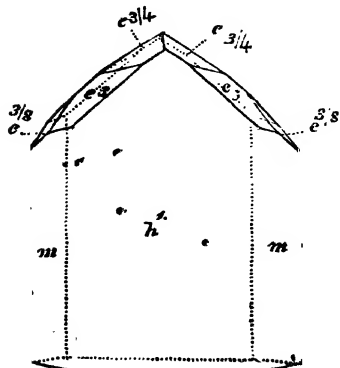
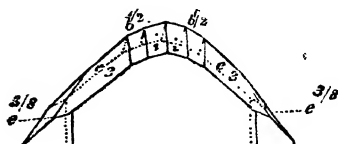


Fig. 3.



measured by the reflecting goniomètre, with the exception of the plane  $h^1$ , which is strongly striated longitudinally. Some of the crystals are opaque, and of a pale red colour; others are translucent and transparent, and of a deep orange red colour, somewhat like the cinquanon stone. Fig. 4 represents a beau-

Fig. 4.

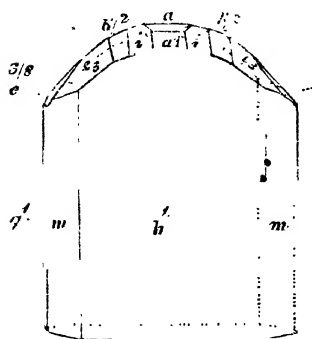


Fig. 5.



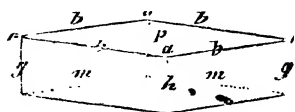
tiful crystal of this colour placed on a group of rock crystal in the collection of Mr. James Sowerby.

Upon a group of rock crystals from Dauphiny, in the collection of Mr. Turner, I observed with lamellar crichtonite some flat very brilliant brown translucent crystals, the form of which is represented by fig. 5, and which belong to the same species as those above described; they present, however, new modifica-

tions which are the planes designated by  $p$ ,  $e_2$ , and  $e^{\frac{1}{4}}$ ; but all the other planes  $m$ ,  $h^1$ ,  $g^1$ , and  $e_1$ , measure exactly the same angles as those marked with the same letters in the crystals from Snowdon.

I have taken for the lateral faces of the primitive form the planes marked  $m$ , which are inclined to one another at an angle equal to  $100^\circ$ , and by assuming also that the planes marked  $e_1$ , the incidence of which upon  $m$  is equal to  $134^\circ$ , is the result of a decrement by three rows in breadth on the lateral angles  $e$  of the primitive, I have found that one side of the base was to the height nearly in the ratio of 30 to 11. A right rhombic prism, fig. 1, of  $100^\circ$ , and of such dimensions, may therefore, be considered as the primitive form of this substance. The other planes are marked with the signs corresponding to the decrements of which they are supposed to be derived, and the incidences calculated from these laws agree within very narrow limits with the observation. The faces

Fig. 1.



marked  $i$  are the result of an intermediary decrement, the sign of which is ( $b^1, b^{\frac{1}{3}}, g^{\frac{1}{3}}$ ).

$m, m$	.....	=	100°	0'
$m, e_3$	.....	=	134	0
$m, e^{\frac{3}{8}}$	.....	=	120	4
$m, e$	.....	=	124	45
$m, e_2$	.....	=	121	58
$m, e^{\frac{1}{4}}$	.....	=	124	31
$m, b^{\frac{1}{2}}$	.....	=	126	0
$p, m$	.....	=	96	0
$p, e_3$	.....	=	132	38
$p, e^{\frac{3}{8}}$	.....	=	128	48
$p, e$	.....	=	141	41
$p, e_2$	.....	=	143	2
$p, e^{\frac{1}{4}}$	.....	=	118	12
$p, b^{\frac{1}{2}}$	.....	=	144	0
$p, a^1$	.....	=	150	56
$p, a^2$	.....	=	164	28
$p, i$	.....	=	147	30
$e_3, e_3$	.....	=	135	46
$e^{\frac{3}{4}}, e^{\frac{3}{4}}$	.....	=	150	0
$e_2, e_2$	.....	=	154	21
$b^{\frac{1}{2}}, e_3$	.....	=	162	58
$e_3, e^{\frac{3}{8}}$	.....	=	156	29
$e_3, i$	.....	=	156	0
$e_3, e_3$	.....	=	101	37
$e^{\frac{3}{4}}, e^{\frac{3}{4}}$	.....	=	109	21
$e_2, e_2$	.....	=	112	3
$i, i$	.....	=	149	35

This substance I propose to call *Brookite*, in honour of Mr. Brooke.

We hope to give the characters of this mineral before the blowpipe, and its chemical analysis, in our next.—C. and P.

## ARTICLE XI.

*Proceedings of Philosophical Societies.*

## ROYAL SOCIETY.

*Dec. 23.*—Two papers by the Rev. Baden Powell, MA. FRS. were read, supplementary to a former communication on Radiant Heat; and the Society adjourned to

*Jan. 13, 1825;* when John Bell, Esq. and William Scoresby, Jun. Esq. were admitted Fellows of the Society; and A Description of a Floating Collimator, by Capt. H. Kater, FRS. was read.

This instrument is destined to supply the place of a level or plumb line in astronomical observations, and to furnish a ready and perfectly exact method of determining the position of the horizontal or zenith point on the limb of a circle or zenith sector. Its principle is the invariability with respect to the horizon of the position assumed by any body of invariable figure and weight floating on a fluid. It consists of a rectangular box containing mercury, on which is floated a mass of cast iron, about twelve inches long, four broad, and half an inch thick, having two short uprights, or Y's, of equal height, cast in one piece with the rest. On these is firmly attached a small telescope furnished with cross wires, or, what is better, crossed portions of the fine balance spring of a watch, set flat-ways, and adjusted *very exactly* in the sidereal focus of its object glass. The float is browned with nitric acid to prevent the adhesion of the mercury, and is prevented from moving laterally by two smoothly polished iron pins, projecting from its sides in the middle of its length, which play freely in vertical grooves of polished iron in the sides of the box. When this instrument is used, it is placed at a short distance from the circle whose horizontal point is to be ascertained on either side (suppose the north) of its centre; and the telescopes of the circle and of the collimator are so adjusted as to lock mutually at each other's cross wires (in the manner lately practised by Messrs. Gauss and Bessel), first of all coarsely by trial, applying the eye to the eye-glasses of the two instruments alternately; and finally by illuminating the cross wires of the collimator with a lanthorn and oiled paper, taking care to exclude false light by a black screen having an aperture equal to that of the collimator, and making the coincidence in the manner of an astronomical observation, by the fine motion of the circle. The microscopes on the limb are then read off, and thus the apparent zenith distance of the collimating point (intersection of the wires) is found. The collimator is then transferred

to the other (south) side of the circle, and a corresponding observation made, *without reversing the circle*, but merely by the motion of the telescope on the limb. The difference of the two zenith distances so read off is double the error of the zenith or horizontal point of the graduation, and their semi-sum is the true zenith distance of the collimating point, or the co-inclination of the axis of the collimating telescope to the horizon.

By the experiments detailed in Capt. Kater's paper, it appears that the error to be feared in the determination of the horizontal point by this instrument can rarely amount to half a second if a mean of four or five observations be taken. In a hundred and fifty-one single trials, two only gave an error of two seconds, and one of these was made with a wooden float. In upwards of a hundred and twenty of these observations, the error was not one second.

For further details, we must refer to the original communication.

Jan. 20.—Capt. F. W. Beechy, RN. was admitted a Fellow of the Society, and the following paper was read:—

On the Construction of the Barometer; by J. F. Daniell, Esq. FRS.

In a former communication to the Royal Society on the Construction of the Barometer, the author had inferred from some experiments therein detailed, that the capillary depression of the mercury in barometer-tubes was decreased one-half by boiling; and the first object of the present paper was to describe some new experiments that he had made on this subject, the results of which confirmed his former deductions. In these the depression of the mercury in tubes of from  $\frac{1}{10}$  to  $\frac{6}{10}$  of an inch internal diameter was measured to the  $\frac{1}{1000}$  part of an inch, by a particular apparatus constructed for the purpose, and described in the paper; and their results very nearly agreed with those given in Dr. Young's tables, calculated from the experiments of Lord Charles Cavendish: on repeating the experiments after boiling mercury in the tubes, Mr. Daniell found the amount of the depression to be one-half of what it was before; as he had formerly concluded.

Mr. Daniell proceeded to detail some facts relating to the gradual deterioration of barometers by the insinuation of air between the mercury and the tube, and to describe the means he had devised for obviating this defect in the instrument. He had been informed that the mercury in the barometer constructed under his superintendence, and set up in the apartments of the Royal Society, by the direction of the Meteorological Committee, exhibited a peculiar speckled appearance; and on examination he found a number of minute bubbles of air between the glass and the mercury, increasing in size towards the top.

In seeking for a method of removing this source of inaccuracy, it occurred to Mr. Daniell that gases were better confined over water than over mercury, on account of the water making a perfect contact with the glass of the jars in which they were contained, which was not the case with the mercury; and Mr. Faraday furnished him with a case in point, in which a mixture of oxygen and hydrogen confined in bottles over water, and in the dark, for about a twelvemonth, were found unaltered either in nature or in quantity; whilst bottles into which the same mixture had been passed, and confined over mercury, under the same circumstances, were found to contain nothing but common air. Mr. D. thence inferred, that if the tube consisted of some substance which the mercury would *wet* (if he might be allowed the expression), the insinuation of air would be prevented. In the experiments he made when constructing a new pyrometer, he had found that platinum immersed in mercury acquired a complete surface of that metal; and now in keeping a strip of platinum foil in mercury for some time, he found that its tenacity was unimpaired. A tube of platinum, of about an inch in length, was accordingly welded to the open end of a barometer-tube, with which the mercury forming a perfect contact, would effectually prevent, it might be presumed, the insinuation of the air: the instrument was then filled, and finished as usual. A mere ring of platinum also, which would be much less expensive, would be equally efficacious, as the smallest surface of perfect contact must be sufficient. As a considerable time, however, must elapse before the success of this method could be shown by the barometer itself, the author had instituted an experiment in which the effect would be sooner apparent;—he had confined a mixture of oxygen and hydrogen over mercury in two jars, one of them having a ring of platinum at its lower extremity. He had not been able to discover in registers of barometrical observations any distinct evidence of the gradual deterioration of barometers from the cause he had thus endeavoured to obviate; the observers, however, having frequently found it necessary, for some reason, either to re-boil the mercury in the tube, or to change their instrument altogether.

#### ASTRONOMICAL SOCIETY.

This Society held its first meeting after the summer recess, on Friday the 12th of November; the President, H. T. Colebrooke, Esq. in the chair. Several new members were elected, and others proposed, and a great number of valuable presents, especially from foreign astronomers, were announced.

Two communications were read from Sir Thomas Brisbane, Governor of New South Wales. The first of these contained an account of some observations made at Paramatta, by Sir Thomas. *New Series*, VOL. IX.

mas, and Mr. Dunlop, on the inferior Conjunction of Venus with the Sun, in October, 1823.

Sir Thomas's second communication, which is dated 17th April, 1824, contains, first, a record of repetitions on the Sun, with Reichenbach's circle, for the Summer Solstice, 1823, they extend from Dec. 10, 1823, to Jan. 2, 1824, but have not yet been subjected to the necessary reductions for a definite result : secondly, a series of observations on several stars, made at Paramatta with the Mural circle, from Nov. 20, 1823, to Feb. 19, 1824. Twenty of the stars observed are among those whose places are given annually in the Nautical Almanac, and are usually denominated Greenwich stars.

A letter was also read from Baron Zach to Francis Baily, Esq. FRS. dated Genoa, July 21, 1824, announcing the discovery of a telescopic comet, by M. Pons, on the 24th of that month. It was in the head of Serpentarius, without tail or coma :—a simple nebosity.

Mr. Herschel submitted to the inspection of the members present, a new double image micrometer, by Prof. Amici, of Modena.

Mr. Donkin laid on the table, for the inspection of the members, an instrument made by M. Faton (a pupil of Breguet, at Paris), for determining the *fractional part* of a second of time, in astronomical observations.

### *Prize Questions proposed by the Astronomical Society of London.*

This Society has just proposed the following prize questions, to the consideration of astronomers and mathematicians, viz.

1. The silver medal to any person who shall contrive, and have executed an instrument, by which the relative magnitude of the stars may be measured or determined ; and of which the utility for this object shall be sufficiently established, by numerous observations, and comparisons of known stars.

2. The gold medal for approved formulæ, for determining the true place of either of the four newly discovered planets, Ceres, Juno, Vesta, and Pallas ; within such limits as the Council may think sufficiently correct for the present state of astronomy ; such formulæ in each case to be accompanied with comparisons of the observed places at various periods.

3. The gold medal for a new mode of developing the differential equation for expressing the problem of the three bodies, by which a *smaller number* of tables shall be required in order to compute the moon's place to the same degree of accuracy, as by any existing tables, and with greater facility.

To be entitled to competition for the prizes, all answers to the first question must be received before the 1st of February, 1826 ; to the second, before the 1st of February, 1827 ; and to the third, before the 1st of February, 1828.

Dec. 10.—At the meeting of the Society this evening, the publication of the second part of their Memoirs was announced.

A paper, drawn up by Dr. Gregory, was read, containing a description of a box of rods, named the *Rhabdological Abacus*, presented to the Society by the family of the late Henry Goodwyn, Esq. of Blackheath. It appears that these rods were invented by Mr. Goodwyn for the purpose of facilitating the multiplication of long numbers of frequent occurrence: they were probably suggested by Napier's Rods, and are, for the purposes which the inventor had in view, a great improvement upon them. The rods, which are square prisms, contain on each side, successively, the proposed number in a multiplicand, and its several multiples up to nine times; and these in the several series of rods are repeated sufficiently often to serve for as extensive multiplications as are likely to occur. Thus if the four faces of one rod contain respectively, once, twice, three times, and four times a proposed multiplicand; another rod will exhibit in like manner two, three, four, and five times the same; a third rod, three, four, five, and six times the same; and so on to nine; and in several cases, more rods. The numbers are arranged uniformly upon equal and equidistant compartments; while at a small constant distance to the left of each product stands the number two, three, four, five, &c. which it represents. Hence, in performing a multiplication, the operator has only to select from the several faces of the rods the distinct products which belong to the respective digits in the multiplier, to place them in due order *above* each other, to add them up while they so stand, and write down their sum, which is evidently the entire product required, and obtained without the labour of multiplying for each separate product, or even of writing those products down. For still greater convenience the rods may be arranged upon a board with two parallel projections placed aslant at such an angle as of necessity produces the right arrangement. There are blank rods to place in those lines which accord with a cypher in the multiplier; and the arrangement may easily be carried on from the bottom product upwards, by means of the indicating digits.

A letter was read from Capt. Ross, a Member of this Society, giving an account of observations made on the occultation of Jupiter by the moon on the 5th of April last; transmitting also an account of observations upon the same occultation by Mr. Ramage, of Aberdeen, with one of his own 25 feet reflecting telescopes. Mr. R. observed the *immersion*. On the approach of Jupiter's satellites to the moon, a diminution of their light was perceptible. On coming into contact with the moon's dark limb, they did not disappear instantly, like fixed stars, but formed an indentation or notch in the limb, as if they were imbedded in it, but were at the same time separated from it by a fine line of light. This indentation continued visible until

about *half* their diameters were immersed, when it disappeared. All the satellites presented this phenomenon; but the fourth and third with the greatest distinctness. On Jupiter's approach, no difference of his light or shape was perceptible, but after the contact had taken place, he appeared to exhibit no deficiency of disc, but presented a complete figure, as if placed between the moon and the earth, this appearance continuing for a few seconds. When the planet was almost entirely immersed, his retiring limb appeared as though it were considerably elongated, or formed a segment of a much larger circle than had been previously presented. The position of Mr. Ramage's telescope did not allow him to observe the emersion.

Capt. Ross was prevented by the state of the weather from seeing the *immersion*, but was fortunate enough to observe the *emersion*, seeing first a considerable *elongation*, which gradually diminished as more of the planet appeared from behind the moon.

Part of a letter was read from Mr. R. Comfield, a Member of this Society, in reference to the same occultation. He observed it at Northampton with a good Newtonian reflector. Mr. Comfield, and two other contemporaneous observers, with good instruments, noticed that when Jupiter had about half disappeared, there was exhibited an adhesion or protuberance on each side of the planet, which, as Jupiter sunk behind the moon, became larger and larger, so that just before the entire disappearance of the planet, it exhibited a considerable elongation deviating greatly from a circular curve of the same diameter as the planet.

Phenomena, somewhat analogous, especially in reference to the indentations and adhesions, were noticed by several astronomers who observed the transit of Venus in 1769. See the account by Capt. Cook, Mr. Charles Green, Mr. Charles Mason, M. Pingré, &c. in the Phil. Trans. for 1770 and 1771, which are here adverted to, because the consideration of kindred phenomena may assist in the explication of the whole.

Jan. 14, 1825.—At the meeting this evening, Mr. Baily laid on the table for the inspection of the members, two micrometers, which have been recently invented and constructed by M. Fraunhofer of Munich.

These micrometers are formed by means of very fine lines, cut on glass with a diamond point in a peculiar manner, and placed in the focus of the telescope. One of these micrometers consists of concentric circular lines drawn at unequal distances from each other; and the other consists of straight lines crossing each other at a given angle. The mode of cutting these lines has furnished M. Fraunhofer with a method of illuminating them, which (at the same time that it renders the lines visible) leaves the other part of the field of the telescope in darkness; so that the transits of the smallest stars may be observed by means of these micrometers; the lines appearing like so many silver

threads suspended in the heavens. A short account of the circumstances which led M. Fraunhofer to this happy invention was read.

An engraving of Fraunhofer's achromatic telescope at Dorpat of 14 feet focus and 9 inches aperture, was also submitted to the inspection of the members by Mr. Herschel.

A communication was read from Capt. Ross, dated Stranraer, Aug. 7, 1824, in which he transmits a diagram exhibiting his observation of the occultation of Herschel's planet by the moon, on the preceding day, with Ranage's 25 feet telescope, and a power of 500. The planet appeared to have entered about one-third of its diameter on the dark part of the moon before it disappeared, and its light began to diminish before it touched the lunar disc. On the contrary at its emersion, it appeared one-fourth of its own diameter distant from the moon's western limb. The whole time of the occultation was 1<sup>h</sup> 7<sup>m</sup> 44.5<sup>s</sup>.

After this the reading was commenced of a paper by Mr. H. Atkinson, of Newcastle-upon-Tyne, "On Astronomical and other Refractions; with a connected Inquiry into the Law of Temperature in different Latitudes and Altitudes." As the reading of this paper will be resumed at a subsequent meeting, an abstract of the whole may with propriety be deferred.

#### GEOLOGICAL SOCIETY.

*Dec. 3.*—A notice was read, "On some Fossils found in the Island of Madeira;" by the late T. E. Bowdich, Esq.

In this notice, the author describes a formation of branched cylindrical tubes encased with agglutinated sand, which occur in great abundance near Fanical, 15 miles from Funchal, in the Island of Madeira. Mr. Bowdich is inclined to refer these to a vegetable origin. They are accompanied by shells, some decidedly terrestrial, and others which appear to belong to a marine genus. In conclusion, some account is given of the general features and structure of the neighbouring district.

An extract of a paper was then read, entitled, "An Inquiry into the Chemical Composition of those Minerals which belong to the genus Tourmaline;" by Dr. C. G. Gmelin, Professor of Chemistry in the University of Tubingen, and For. Mem. GS.

Prof. Gmelin, in this memoir, details at length, the various analyses of minerals of the Tourmaline family, which have been made by former chemists. He then describes the methods which he adopted in his own experiments, and adds the results which he obtained from them.

The author divides the different species of Tourmaline into the following sections: 1. Tourmalines which contain lithion; 2. Tourmalines which contain potash or soda, or both these alkalis together, without lithion, and without a considerable quantity of magnesia; 3. Tourmalines which contain a considerable

quantity of magnesia, together with some potash, or potash and soda.

It appears, he says, in conclusion, that when we compare the analyses of the different species of Tourmalines, the most essential ingredients are, boracic acid, silica, and alumina, whose relative quantities do not vary much. It appears further, that any alkaline substance, though in no considerable quantity, may be likewise an essential ingredient. The different nature of these alkaline substances may be employed by the chemist, as we have used it, to divide these minerals into different sections. But it will appear to be quite useless to attempt to give mineralogical formulæ for the chemical composition of these minerals, when it is considered; first, that we can by no means rely upon the correctness of any statement regarding the quantity of oxygen in boracic acid; secondly, that the quantity of alkaline bases, whose oxygen would be unity, is so small, that it cannot be determined (with sufficient accuracy) without great errors in the computation of the relative quantity of oxygen in the other ingredients; thirdly, that in one species no account could be given of a considerable loss of weight. He has, however, calculated the quantities of oxygen in every species, with the intention of comparing the sum of the oxygen contained in the bases with the sum of that contained in the acids, viz. boracic acid and silica. The result of this calculation is then fully stated.

## ARTICLE XII.

### SCIENTIFIC NOTICES.

#### CHEMISTRY.

##### 1. *Analysis of the Boletus Sulphureus.*

THIS mushroom, according to Peschier's analysis, is composed of the following ingredients:—

Water,

Fungin,

Albumen,

An uncrystallizable saccharine matter—mushroom sugar,

A fatty substance soluble in alcohol,

An animal matter,

A peculiar alkaline principle,

Oxalate of potash,

An uncombined acid of a peculiar nature, and

A colouring matter.

The uncombined acid and the colouring matter were soluble both in water and in alcohol.—(Trommsdorff's *Neues Journal der Pharmacie.*)

## 2. Compound of Muriate and Hydrosulphuret of Oxidule of Antimony.

Sulphuretted hydrogen throws down from a solution of the muriate of oxidule of antimony a lively pomegranate yellow coloured precipitate, which has been hitherto regarded as a pure hydrosulphuret of oxidule of antimony: it is, however, a combination of this salt with the neutral muriate of oxidule of antimony. The latter salt may be expelled by heat, and sulphuret of antimony remains behind; the same decomposition may be effected by exposing the precipitate for some time in a close vessel to the light of the sun.—(L. Gmelin. Handbuch der theoretischen Chemie.)

## 3. Composition of White Precipitate.

We copy the following from a note, at the conclusion of Mr. Brande's paper, entitled "Facts towards the Chemical History of Mercury:"

Having inferred from various experiments that the "white precipitate" was a compound of one proportional of peroxide of mercury, and one of muriate of ammonia, Mr. Hennel verified his opinion as follows: A solution of one proportional of corrosive sublimate ( $= 272$ ) was mixed with a quantity of solution of ammonia, containing two proportionals ( $17 \times 2 = 34$ ) of that alkali; a neutral mixture resulted, white precipitate was formed, and one proportional of muriate of ammonia (ammonia  $17 +$  muriatic acid  $37 = 54$  of muriate of ammonia) was found in solution. In this case, the two proportionals of chlorine in the sublimate ( $36 \times 2 = 72$ ) were converted at the expense of 2 proportionals of water, into 2 of muriatic acid, which, uniting with the ammonia, formed 2 of muriate of ammonia. The 2 proportionals of the oxygen from the water (equivalent to the 2 of hydrogen transferred to the chlorine) united to the 1 proportional of mercury in the sublimate, to form 1 of peroxide of mercury, which fell in combination with 1 of muriate of ammonia to constitute white precipitate; while the other proportional of muriate remained, as above stated, in solution. The equivalent number, therefore, of white precipitate, is 270, and it consists of

1	proportional of peroxide of mercury. . .	=	216	....	80
1	" " " muriate of ammonia. . .	=	54	....	20
			<hr/>		<hr/>
			270		100

Having thus synthetically established the composition of white precipitate, the following analytical experiment was made upon it: 270 grains were dissolved in hydrocyanic acid, and sulphuretted hydrogen was passed through the solution till it occasioned no further change; the precipitate was then col-

lected, washed, and dried; it weighed very nearly 232 grains, being the equivalent of bisulphuret of mercury. The filtered liquor, on evaporation to dryness, left 54 grains, or 1 proportional of muriate of ammonia.—(Journal of Science.)

#### 4. *Boron, its Preparation, &c.*

The readiest method of obtaining boron without losing too much potassium is to heat the potassium with fluo-borate of potash.\* Boron and silicium resemble each other in their properties, nearly as sulphur and silicium, or as phosphorus and arsenic. I have produced sulphuret of boron, a white and pulverulent substance, which dissolves in water, yielding sulphuretted hydrogen gas. Boron burns in chlorine. The chloride of boron is a permanent gas which is decomposed in moist air, producing a dense vapour; and in water giving muriatic and boracic acids. It condenses one and a half time its volume of ammoniacal gas. *Berzelius. Bib. Univ.*—(Journal of Science.)

#### 5. *Action of Alum on Vegetable Blue Colours.*

It is commonly stated in chemical works, that a solution of alum has the property of reddening vegetable colours. With the exception of litmus, where the effect is very *decided*, and of tincture of cabbage, where the effect is *trifling*, a contrary effect is experienced; the solution has turned the colours (which were generally obtained from the blue petals of flowers) green. *H. B. Leksou.*—(Journal of Science.)

#### 6. *Preparation of Lithia.*

M. Berzelius says, that the most economical way of preparing lithia is to mix the triphane, or spodumene, in powder, with twice its weight of pulverised fluor spar, and with sulphuric acid; then to heat the mixture until the fluoric acid with the silica is volatilized, and afterwards to separate the sulphate by solution. *Bib. Univ.*—(Journal of Science.)

#### 7. *On Sulpho-iodide of Antimony.* By MM. Henry and Garot.

When very dry iodine and sulphuret of antimony are mixed in equal parts, and sublimed in dry vessels by the moderated heat of a sand-bath, red vapours appear, which condense on the upper and cooler parts of the vessels, whilst a greenish grey mixture of protoxide of antimony with a little iodide and sulphuret remains.

The condensed volatile substance appears in brilliant translucent plates, resembling fern-leaves in form, of an intense poppy red colour: if the vessels in which the sublimation has been made are large, the crystals appear as prismatic prisms. When heated, they readily fuse, and by careful management may be

\* See Preparation of Silicium, *Annals*, vol. viii. p. 122, New Series.

repeatedly sublimed; but when highly heated, iodide and sulphur are set free, sulphurous acid is formed, and a mixture of antimony and oxide produced. The crystals have a sharp disagreeable taste: light has no action on them. When put into alcohol or ether, iodine is dissolved, and a yellow sulphuret of antimony deposited. When put into water, hydriodic acid, protoxide of antimony, and sulphur, are formed. The action of the acids is such as might be expected, decomposition of the substance being always produced.

Upon analysis, this substance gave as its elements, antimony 23·2, iodine 67·9, sulphur 8·9, which nearly corresponds with one proportional of each substance. The authors have called it a sulpho-iodide of antimony. *Jour. de Pharm.*—(Journal of Science.)

#### MINERALOGY.

##### 8. *Yenite found in the United States.*

Dr. Torrey states, that a mineral has been found at Rhode Island, which, from its characters, he considers as yenite. It is in small crystals imbedded in an aggregate of quartz and epidote. The crystals vary in size; the largest found was an inch and a quarter long, one quarter of an inch broad, and two lines thick. The terminations were wanting. The form is nearly rectangular; the surface striated and shining, with a semi-metallic lustre. Cross fracture somewhat resinous. It is imperfectly foliated in the direction of the longer diagonal of the prism. It scratches glass slightly. It is opaque, and of a blackish brown colour. The powder has the colour of the mass. Specific gravity 3·6.

Before the blowpipe, it melts with great ease into a black opaque glass, strongly attracted by the magnet.—(*Annals of Lyceum of Natural History, New York.*)

##### 9. *Localities of rare Minerals.*

Chrome ore, the chromate of iron, has been discovered by Sir Humphry Davy in small granular masses, disseminated in a greenish-white marble from Buchanan, in Stirlingshire, preserved in Mr. Allan's cabinet. Of the Cronstedtite, of Steinmann, a mineral hitherto confined to Przibram, the same collection contains specimens from Wheal Maudlin, in Cornwall. The cronstedtite from the latter locality presents generally thinner individuals than the Bohemian one, but is, like this, accompanied by sparry iron and hexahedral iron pyrites. Another product of Wheal Maudlin has lately attracted the attention of mineralogists. The collections of Mr. Allan, Mr. Rashleigh, of Menabilly, and Mr. Williams, of Scorrier, contain pseudomorphous crystals of wolfram, in the shape of tungstate of lime. They present the form, well known in that species, of an isosceles four-sided pyramid, bevelled on the solid angles

contiguous to the base, sometimes of the size of three or four lines in every direction. They are generally engaged in blende, which is cleavable in large laminae, and are composed in the interior of a delicate tissue of minute crystals, between which numerous cavities are conspicuous, lined with these crystals. Sometimes also the large pseudomorphous crystals are quite disengaged, and accompanied by arsenical pyrites, chlorite, quartz, &c. The colour of the streak is almost of the same colour in the pseudomorphous crystals, and the blende in which they are imbedded.—(Edinburgh Journal of Science.)

#### 10. *English Locality of Metallic Lead.*

This substance has lately been found *in situ* in the neighbourhood of Alston. It occurs in small globular masses, imbedded in galena and a slaggy substance, accompanied with red litharge, crystals of blende and quartz. The vein in which it is found is in limestone, and of the thickness of an inch, widening out to two or three as it goes down. The whole mass within the vein is considerably decomposed, and the ore is found in incoherent pieces, some of which are about the size of a walnut. Many of them have a very slaggy appearance, both externally and internally, while others are pure galena, distinctly cleavable, and coated with a white mealy sulphate of lead, produced by decomposition. A more particular notice of this mineral will probably soon be given.—(Edin. Jour. of Science.)

#### MAGNETISM.

#### 11. *Gay-Lussac on the mutual Action of two Magnetic Particles in different Bodies.*

This very interesting experiment was undertaken by M. Gay-Lussac at the request of M. Poisson, for the purpose of ascertaining whether or not the mutual action of two magnetic particles depended on the matter of each of the bodies, which was found to be the case.

A magnetical needle, eight inches long, was found to make ten horizontal vibrations near the direction of the magnetic meridian in 131 seconds. A prismatic bar of soft iron, about eight inches long, three-fourths of an inch wide, and one-eighteenth of an inch thick, in a vertical direction, was now fixed at the distance of two inches below the needle, and in the plane of the magnetic meridian. The oscillations of the needle became more frequent, being about 10 in 65 seconds, and soon after 10 in 60 seconds.

A similar and equal bar of pure nickel was now substituted in place of the iron bar, and the needle made at first 10 oscillations in 78 seconds, and soon after 10 in 77 seconds. When the bar of nickel was removed, the needle made 10 oscillations in 130 seconds by the action of the earth alone. *M. Poisson's Memoir on Magnetism.*—(Edin. Jour. of Science.)

## MISCELLANEOUS.

12. *Hydrophobia.*

Dr. Capello, of Rome, in a memoir read before the Academy del Lincei, affirms that the hydrophobic poison, after its first transmission, loses the power of conveying the disease. This observation, already made by Bader, is confirmed by repeated experiments made by Dr. Capello. A lap-dog and cat were both inoculated with the saliva of a dog who died of inoculated hydrophobia; they both remained free from disease; and three years afterwards the lap-dog was again inoculated from a dog who became rabid spontaneously: he then took the disease and died.

An ox was bitten by a dog attacked with rabies; he became hydrophobic, and bit many other animals: all remained free from the affection. The dog that bit the ox also bit a child, who died about four months after, with all the symptoms of hydrophobia: with the saliva of this child a dog also was inoculated, but the disease was not transmitted.

A dog which had been bitten by another dog became hydrophobic on the fifty-first day, broke the chain with which he was fastened, and escaped into the street, where he bit many persons, and the dogs of two persons (who are named), and finally disappeared among the ruins of the villa of Quintilius Varus: not one of the persons or dogs so bitten had the slightest symptom of hydrophobia. *Med. Jour.*—(Journal of Science.)

13. *Temperature of the Maximum Density of Water.*

An elaborate memoir by Prof. Hållström, on the specific gravity of water at different temperatures, and on the temperature of its maximum density, has appeared in the Swedish Transactions for 1823. It is divided into two parts: The first contains a critical discussion of the results, and the methods employed by preceding experimenters: the second, a detail of an extensive course of experiments, instituted by himself, with a view to the more accurate determination of this important but difficult inquiry. The method of experimenting which he regarded as the most accurate, and which he therefore adopted, was to ascertain the weight of a hollow glass globe, very little heavier than water, and about  $2\frac{1}{4}$  inches in diameter, in water of every degree of temperature between  $0^{\circ}$  and  $32.5^{\circ}$  cent. The errors arising from a dilatation or contraction of the glass, the weight of the atmosphere, &c. were all calculated, and a corresponding correction made. The result was, that water attains its greatest density at a temperature of  $4.108^{\circ}$  cent. ( $39.394^{\circ}$  Fahr.); and the *limits of uncertainty*, occasioned by the impossibility of ascertaining the dilatation of glass with perfect accuracy, he estimates to be  $0.238^{\circ}$  ( $0.428^{\circ}$  Fahr.) on either side of this number.

The two following tables exhibit the results of his experiments on the sp. gr. of water in all temperatures between  $0^{\circ}$  and  $32^{\circ}$  cent. In the first, the sp. gr. at  $0^{\circ}$ ; in the second, the sp. gr. at  $4.1^{\circ}$  is taken as the unit.

Temp.	Sp. Gr.	Temp.	Sp. Gr.	Temp.	Sp. Gr.
Cent.		Cent.		Cent.	
$0^{\circ}$	1.0000000	$10^{\circ}$	0.9998906	$21^{\circ}$	0.9983648
1	1.0000163	11	0.9998112	22	0.9981569
2	1.0000799	12	0.9997196	23	0.9979379
3	1.0001004	13	0.9996160	24	0.9977077
4	1.00010817	14	0.9995005	25	0.9974666
4.1	1.00010824	15	0.9993731	26	0.9972146
5	1.0001032	16	0.9992340	27	0.9969518
6	1.0000856	17	0.9990832	28	0.9966783
7	1.0000555	18	0.9989207	29	0.9963941
8	1.0000129	19	0.9987468	30	0.9960993
9	0.9999579	20	0.9985615		

Temp.	Sp. Gr.	Temp.	Sp. Gr.	Temp.	Sp. Gr.
Cent.		Cent.		Cent.	
$0^{\circ}$	0.9998918	$10^{\circ}$	0.9997825	$21^{\circ}$	0.9982570
1	0.9999382	11	0.9997030	22	0.9980189
2	0.9999717	12	0.9996117	23	0.9978300
3	0.9999920	13	0.9995080	24	0.9976000
4	0.9999995	14	0.9993922	25	0.9973587
4.1	1.0000000	15	0.9992647	26	0.9971070
5	0.9999950	16	0.9991260	27	0.9968439
6	0.9999772	17	0.9989752	28	0.9965704
7	0.9999472	18	0.9988125	29	0.9962864
8	0.9999044	19	0.9986387	30	0.9959917
9	0.9998497	20	0.9984534		

The uncertainty which still exists respecting the temperature of the maximum density of water may, perhaps, be best illustrated by a table of the results which he brings successively under review.

Observer.	Calculator.	Observer.	Calculator.	
				Cent.
De Luc.	Biot.	Charles.	Biot.	$3.99^{\circ}$
	Ekstrand.		Paucker.	3.88
	Paucker.	Lefevre-Gineau.	Lefevre-Gineau.	4.44
	Hällström.	Hällström.	Hällström.	4.35
Dalton.	Dalton.	Bischof.	Bischof.	4.06
	Biot.	Rumford.	Rumford.	4.38
Gilpin.	Young.		—	3.47
	Biot.	Tralles.	Tralles.	4.35
	Eytelwein.	Hope.	Hope.	3.33
	Walbeck.		—	3.88
	Hällström.		—	4.16
Schmidt.	Eytelwein.	Ekstrand.	Ekstrand.	3.60
	Hällström.		—	3.90

Before commencing his investigation, Prof. H. determined in the first place the dilatation of the glass which he employed in the course of his experiments. His results, particularly in the two extremes of temperature, differ considerably from those of Lavoisier and General Roy; on which account we consider it worth while to insert them here.

Temperature.	Expansion.	Temperature.	Expansion.
Cent.		Cent.	
0°	0.000000	60°	0.000496
10	0.000030	70	0.000652
20	0.000081	80	0.000829
30	0.000153	90	0.001027
40	0.000246	100	0.001246
50	0.000361		

#### 14. Prof. Oersted on a Method of accelerating the Distillation of Liquids.

In Gehlen's *Journal für Chemie und Physik*, i. 277—289, I have related a few experiments which demonstrate that the disengagement of gas in a fluid, resulting from chemical decomposition, never takes place except in contact with some solid body. This principle may without doubt be applied to the disengagement of vapours. If a metallic wire be suspended in a boiling fluid, it instantly becomes covered with bubbles of vapour. Hence it might be concluded that a large number of metallic wires, introduced into a fluid which we wish to distil, would accelerate the formation of vapours. To prove this opinion, I introduced 10 pounds of brass wire, of one-fifth of a line in diameter, loosely rolled up, into a distillatory vessel containing 20 measures (about 10 pints) of brandy: the result was, that seven measures of brandy distilled over with a heat, which, without the wire, was capable of sending over only four measures.

An expedient similar to this has been long in common use in England. When a steam-boiler has become encrusted with so much earthy matter that the contained water ceases to boil with rapidity, it is customary to throw in a quantity of the residue obtained from malt by extracting its soluble portion, and which consists chiefly of small grains or fibres. Here the disengagement of vapour is promoted by the large number of thin and solid particles.—(*Tidskrift för Naturvidenskaberne.*)

Prof. Oersted's information respecting the latter method of promoting the generation of vapour, was probably derived from a paper by Mr. Bald, in the *Edin. Phil. Journ.* vol. ii. p. 340. The material which the engine-keepers of Scotland are in the constant practice of employing to produce this effect, is not, as

M. Oersted states, the exhausted portion of malt; although there seems no reason to doubt that it, and indeed any light substance, in a state of minute division, would prove of nearly equal efficacy. "The substance employed," says Mr. Bald, "is known by the name of *comings*, being the radicles of barley produced in the process of maling, which are separated before the malt is sent to market. About a bushel of these is thrown into the boiler; and when the steam is again raised, an immediate effect is visible; for there is not only a plentiful supply of steam to produce the full working speed of the engine, but an excess of it going waste at the safety valve. This singular effect will continue for several days."

### ARTICLE XIII.

#### NEW PATENTS.

J. Apsden, Leeds, bricklayer, for his improvement in the modes of producing an artificial stone.—Oct. 21, 1824.

G. Dodd, St. Anne-street, Westminster, engineer, for improvements on fire-extinguishing machinery.—Oct. 21.

G. S. Harris, Caroline-place, Knightsbridge, for his machine for the purpose of giving the most effectual and extensive publicity by day and by night to all proclamations, notices, legal advertisements, and which will henceforward render unnecessary the defacement of walls and houses by bill-sticking, placarding, and chalking.—Oct. 21.

J. Lingford, Nottingham, lace-machine manufacturer, for certain improvements upon machines now in use for the purpose of making that kind of lace commonly known by the name of bobbin-net, Buckingham lace-net.—Nov. 1.

Rev. J. Somerville, Edinburgh, for the prevention of all accidental discharge of fowling-pieces or other fire-arms.—Nov. 4.

J. Crosley, Cottage-lane, City-road, for better ensuring the egress of smoke and rarefied air in certain situations.—Nov. 4.

T. R. Guppy, Bristol, for certain improvements in masting vessels.—Nov. 4.

J. Head, Banbury, Oxfordshire, hosier, for improvements in machinery for making cords or plait for boot and stay laces.—Nov. 4.

W. Church, Birmingham, for improvements on augers and bits for boring, and in the apparatus for making the same.—Nov. 4.

W. Busk, Broad-street, for improvements in propelling ships, boats, or other vessels, or floating bodies.—Nov. 4.

J. White and T. Sowerby, both of Bishop Wearmouth, Durham, merchants, for their improved air furnaces for the purpose of melting or fusing metallic substances.—Nov. 6.

J. Modre, Broad Weir, Bristol, for improvements upon steam-engines, or steam-engine apparatus.—Nov. 6.

T. Cartmell, Doncaster, gun-maker, for an improved cock to be applied to the lock of any gun, pistol, fire-arms, or ordnance, for the purpose of firing the same by percussion.—Nov. 6.

## ARTICLE XIV.

## METEOROLOGICAL TABLE.

1824.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.
		Max.	Min.	Max.	Min.		
12th Mon.							
Dec. 1	W	29.78	29.47	43	30	—	
2	S W	29.78	29.63	45	32	—	45
3	N W	29.72	29.65	38	30	—	06
4	N E	29.75	29.72	38	31	—	55
5	N	30.02	29.75	40	27	—	
6	S W	30.00	29.72	44	31	—	18
7	W	30.03	29.72	42	35	—	
8	W	30.03	29.87	46	32	—	06
9	W	30.10	29.87	43	28	—	20
10	N W	30.28	30.10	42	24	—	
11	S W	30.42	30.28	42	38	—	02
12	S W	30.54	30.42	46	38	—	
13	N W	30.59	30.54	48	42	—	
14	W	30.60	30.26	48	41	.34	01
15	S W	30.26	30.08	48	38	—	05
16	W	30.18	30.08	44	35	—	
17	N W	30.31	30.18	42	37	—	06
18	S	30.31	30.25	47	42	—	
19	W	30.23	29.60	52	45	—	—
20	S W	29.63	29.60	53	33	—	04
21	S W	29.62	29.13	52	45	—	10
22	S W	29.95	29.02	50	26	—	15
23	N W	30.11	29.62	52	31	—	35
24	S W	29.75	29.62	50	39	.45	20
25	W	30.04	29.75	54	32	—	
26	W	30.10	30.04	48	35	—	
27	S W	30.04	29.96	52	42	—	—
28	S W	30.27	29.96	54	32	—	50
29	N	30.31	30.27	54	31	—	06
30	W	30.37	30.31	53	44	—	—
31	W	30.33	30.25	54	42	.45	06
		30.60	29.02	54	24	1.24	3.10

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

## REMARKS.

*Twelfth Month.*—1. Fine. 2. Fine morning: very rainy night. 3. Cloudy: rainy night. 4. Rainy. 5. Overcast. 6. Hoar frost: fine day. 7, 8. Fine. 9. Fine day: rainy night. 10. Fine. 11. Drizzling. 12. Fine. 13. Gloomy. 14. Very dark morning: gloomy day: drizzling evening. 15. Cloudy and fine: rain at night. 16. Fine. 17, 18. Drizzling. 19. Cloudy. 20. Squally. 21. Stormy. 22. Squally: an extraordinary rise of the barometer in the night. 23. Very fine. 24. Rainy morning, with high wind: squally day. 25. Rainy morning: fine afternoon. 26. Fine. 27. Fine. 28. Rainy. 29. Very fine day. 30. Cloudy. 31. Drizzling.

## RESULTS.

Winds: N, 2; NE, 1; S, 1; SW, 11; W, 11; NW, 5.

Barometer: Mean height

For the month..... 30.002 inches.  
 For the lunar period, ending the 13th..... 29.8  
 For 13 days, ending the 12th (moon north)..... 29.911  
 For 14 days, ending the 26th (moon south). .... 29.908

Thermometer: Mean height

For the month..... 41.258°  
 For the lunar period, ending the 13th..... 42.155  
 For 29 days, the sun in Sagittarius ..... 41.259

Evaporation..... 1.24 in.

Rain ..... 3.10

# ANNALS OF PHILOSOPHY.

MARCH, 1825.

## ARTICLE I.

*On the Life and Writings of Claude-Louis Berthollet.*  
By Mr. Hugh Colquhoun.

(Concluded from p. 96.)

AFTER seeing the important services which the distinguished friends Berthollet and Monge rendered to France in the early years of her revolutionary warfare, the public confidence which the men of science then generally acquired, and the political weight which in a manner devolved upon them, nothing seems more natural than that men like these friends, who were as remarkable for their taste as for their talent, should be deputed by the Directory in 1796, on the occasion of that brilliant and wonderful campaign of Bonaparte which completely subjugated Italy, to proceed to that land, and select those works of science and art, with which the Louvre was to be filled and adorned. While engaged in the prosecution of that duty, they became acquainted with the victorious general. To know such men was to esteem them: and Napoleon had penetration enough to feel how important their friendship might ultimately prove. He, therefore, cultivated their acquaintance, and was happy afterwards to possess them, with nearly a hundred other philosophers, as his companions in the next expedition which he undertook. In this instance, he no doubt expected that his conquests, and their researches, while they both redounded to the honour of their country, would also equally tend to surround the Commander-in-Chief with an éclat which might favour the development of his schemes of future greatness. This expedition was the invasion of Egypt.

This attempt, which had long been regarded with a favourable eye by the French cabinet, as likely to furnish, after a few brilliant yet easy victories, a soil which should become colonized by and tributary to France, and which promised to produce rice, sugar, corn, and coffee, in abundance, to the injury of the Indian possessions of Britain, was at length in 1798 embarked in, under

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the command of Napoleon, or as he was at that time styled, the General of the Army of Italy. Interesting as the result of the expedition was to the political affairs of Europe, it was not less so to the scientific world. Many of the most illustrious names in France, in every department of philosophy, Berthollet, Monge, Andréossi, Denon, Malus, Descostils, Lévassieur, Fourier, &c. accompanied the army to that country which had been the cradle of so many useful arts and of as much knowledge, and with them the light of science once more shone on the splendid remains of ancient Egypt. In order the more effectually to co-operate in the cause of knowledge, these gentlemen formed themselves into a society named the "Institute of Egypt," which was constituted on precisely the same plan with that of the National Institute at Paris. Their first meeting was on 6 Fructidor, sixth year of the Republic (1798), and after that they continued to assemble at stated intervals: on each occasion, memoirs were read by the respective members, of which, the climate, the inhabitants, and the natural and artificial products of the country they had just entered, together with its antiquities, formed important subjects. After their return to France, there was published in 1800 a highly interesting volume of Memoirs of the Institute of Egypt,—a work to which the names above quoted were the chief contributors.

It is by no means one of the least interesting portions of the history of our chemist, that in which he became intimate with the most extraordinary character of modern times, and in which he is found to be the principal agent in assembling that distinguished company of savans who afterwards formed themselves into the Egyptian Institute. Napoleon, during his occasional intercourse with Berthollet in Italy, had been alike captivated by so great a simplicity of manners, joined to such force and depth of thinking, as he soon perceived to characterise the chemist. When, therefore, he soon after returned to Paris, where he enjoyed a few months of comparative leisure, amid the caresses and admiration of all ranks in the state, he resolved to employ the time of which he had then the disposal, in studying chemistry under Berthollet. It was now that this illustrious pupil imparted to the philosopher his purposed expedition to Egypt, of which no whisper was to be spread abroad until the blow was ready to fall, and begged him, at the same time, not merely to accompany the army himself, but to choose such men of talent and experience as he conceived fitted to find there an employment worthy of the country which they visited, and of that which sent them forth. For Berthollet to invite men to undertake a hazardous expedition, the nature and destination of which he was not permitted to unfold to them, was rather a difficult and delicate task, which however he earnestly undertook. All that he dared say to those whom he engaged in the enterprise, was simply, in the

emphatic words of Cuvier, *Je serai avec vous* ; and never was there a more perfect proof of esteem and affection given, by the universal assent of men of science, to any individual, than those distinguished associates now freely accorded to Berthollet, in pledging themselves to encounter those dangers of which they knew nothing, but that he was to share them. But for the existence of such a man as Berthollet, who possessed at once the entire confidence of the General, and the perfect esteem and regard of men of science, it must have proved wholly impossible to unite on this occasion the advancement of knowledge with the progress of the French arms.

One of the most important essays furnished by Berthollet to the Institute of Egypt, resulted from an investigation into the nature of certain phenomena presented by the Natron Lakes in the neighbourhood of Cairo, situated on the borders of the Desert, and giving name to the Valley of the Six Lakes. The beds of these bodies of water appear to be generally composed of calcareous rock, and the water itself is more or less brackish, in consequence of the presence of a saline matter almost entirely consisting of common salt. These lakes, although extensive, are generally shallow ; and although annually filled to overflowing, they are rapidly dried up again to a large extent, in consequence of the high temperature and remarkable dryness of the climate. As the water retires, it deposits over the whole surface of the country an inexhaustible supply of a hard, compact, saline concretion, consisting of a mixture of carbonate and muriate of soda. This substance contains so much of the former of these salts, that it is extremely valuable for every purpose to which that alkali can be separately applied. Accordingly, immense quantities of it are annually collected under the superintendence of government, and it is not only distributed over the country in caravans, but was at one time exported in great quantities to France, England, Italy, and other parts of Europe. The origin of this carbonate of soda was a question of much interest, but one the resolution of which was attended with no small difficulty.

The water in its original state contains little else than muriate of soda : during evaporation, a quantity of this salt disappears, and is replaced by carbonate of soda. What is the cause of this change ? It should seem that it must be the result of a decomposition of part of the dissolved muriate of soda ; yet what is the manner in which this decomposition is effected ?

It was to attempt the solution of this interesting problem that Berthollet accompanied Andréossy, in the survey which that officer was taking of the Natron Lakes and of the adjacent country. Upon examining carefully the bed of the Lakes, in the hope that some light might thereby be thrown on the object of his research, M. Berthollet made the important observation that

it consisted chiefly of the carbonate of lime, and this led him at once to the true source of the carbonate of soda. He immediately conjectured, and was soon after enabled most luminously to demonstrate, that this salt originates in a double decomposition, which takes place to a partial extent between the carbonate of lime and the muriate of soda: it does not occur when at the ordinary temperatures water impregnated with common salt filters through the pores of carbonate of lime; but Berthollet showed in the most convincing manner that in this instance it is the effect of the peculiar situations to which these two bodies are exposed at the Natron Lakes.

The data on which he founded his opinion were extremely simple. He asserted first that there must exist a mixture of the substances carbonate of lime and muriate of soda; and this mixture cannot but be formed to a certain extent so soon as the water of the Lakes has evaporated, so as to leave a part of its original bed dry. He asserted second, that there must exist a pretty constant though irregular moistening of this mixture with water. Experience proves this also to be the case. Under these circumstances he showed that a portion of the muriate of soda must invariably be converted into carbonate, in consequence of a decomposition taking place between it and the carbonate of lime, the want of energy of the latter being compensated by its proportionally greater mass.

On this occasion our chemist was again greatly instrumental in teaching his country how to avail herself of one of her most valuable resources, which had nevertheless remained till now nearly unknown, and of very partial use. All the carbonate of soda consumed in her bleachfields, her glass, soap, and other manufactories in such quantities, had hitherto been constantly imported from abroad, or had been extracted from barilla, at a comparatively greater expence. Whilst, therefore, it is true that Le Blanc had the merit of first attempting in France the manufacture of this substance out of the muriate of soda or sea salt, yet it was only after the views furnished by Berthollet, after the practical application which he made of the knowledge he had acquired, that the formation of the carbonate of soda from sea-salt, by processes analogous to those which nature employs in Egypt, became universally practised in that country. From that time, however, she has constantly supplied herself from a mine wholly inexhaustible, but which she knew not previously how to work, with all the immense quantity of that useful alkali which she daily consumes. To call this revenue out of what had previously yielded absolutely nothing, and from a quarter which remains for ever ready to furnish an abundant supply, is not to give a beneficial direction to commerce, but absolutely to create a national wealth. The sum of money thus annually saved to France has been computed at more than 40,000,000 of livres. Here again the prosperity and the arts of

his country seem to follow in the train of Berthollet's scientific research, and to spring up and flourish at his command.

The visit to the Natron Lakes was thus productive of two great advantages. It benefitted science by furnishing the solution of what had long been a very interesting but a very puzzling problem, and it furnished a new and useful process to the arts of every commercial country having immediate access to the sea. But besides these, it was attended by a third result, contrasted with which the other two may be almost said to lose their comparative importance. This is a strong expression, but few philosophers will not account it a just one when they are informed that the new views which forced themselves on the rich and original mind of Berthollet, as he studied the progress of the phenomena of the Natron Lakes, in order to explain the manner in which the two salts act upon and decompose each other, opened up to him a train of ideas, which after being matured and digested, formed the subject of the profoundest work that has hitherto appeared on the nature of chemical affinity. The outlines of this work were already sketched, and indeed its leading doctrines were nearly developed ere Berthollet left Egypt. He read a memoir on the subject to the Institute of Egypt, which he published immediately on his return to France; and after many interesting experiments, and much additional illustration and development of his principles, the work itself appeared a few years after under the title of *Statique Chimique*.

During the whole of this expedition, Berthollet and Monge again distinguished themselves by their firm friendship for each other, and by their mutually braving every danger to which any of the common soldiers could be exposed. Indeed, so intimate was their association, that many of the army conceived Berthollet and Monge to be one individual, and it is no small proof of the intimacy of these two savans with Napoleon, when it is learned that the soldiers had a dislike at this corporate personage, from a persuasion that it was at his suggestion they had been led into a country which they detested.

It more than once occurred in the course of the campaign, that Berthollet's courage and integrity were put to a severe test; and it is gratifying to reflect upon the manner in which he acquitted himself. It happened on one occasion that a boat in which he and several others were conveyed up the Nile, was assailed by a troop of Mamelukes, who poured their small shot into it from the banks. In the midst of this perilous voyage, M. Berthollet began very coolly to pick up stones and stuff his pockets with them. When his motive for this conduct was asked, "I am desirous," said he, "that in the case of my being shot, my body may sink at once to the bottom of this river, and may so escape the insults of these barbarians."

On a conjuncture when courage of a rarer kind was required 1,

Berthollet was not found wanting. The plague broke out in the French army, and this, added to the many fatigues they had previously endured, the diseases under which they were already labouring causing the loss of the eyes and of other members, it was feared might either lead to insurrection on the one hand, or totally sink the spirits of the men into despair on the other. But Acre was not yet taken, the expedition had accomplished nothing of permanent advantage, and the general was anxious to dissemble to himself, and to conceal from his troops, the fatal intelligence. When the opinion of M. Berthollet was however asked in council, he spoke at once the plain and simple, though unwelcome truth. He was assailed immediately by the most violent reproaches. "In a week," said he, "my opinion will be unfortunately but too well vindicated." It was as he foretold; and when nothing but a hasty retreat could save the wretched remains of the army of Egypt, the carriage of Berthollet was seized for the convenience of some wounded officers; immediately upon which, and without the smallest discomposure, he travelled on foot across twenty leagues of the desert.

Napoleon knew to appreciate character; and the conduct of Berthollet, even when most contrary to his wishes, had ever commanded his esteem. Once more, therefore, they were companions in that most hazardous voyage in which Napoleon traversed half the Mediterranean in a single vessel, at a time when it was scoured by our fleet, and arrived in France to effect an instantaneous revolution in the government. Long afterwards, when he had attained to the highest pitch of power, however immersed he might be in state affairs, he never forgot his associate Berthollet. He was in the habit of placing all chemical discoveries to his account, to the frequent annoyance of our chemist, and when an unsatisfactory answer was given to him on any scientific subject, he was in the habit of saying, "Well, I shall ask this of Berthollet." Napoleon did not, however, limit his affection to these, however striking proofs of his regard; but having been informed that Berthollet's earnest pursuit of science had led him to so much expenditure as considerably to embarrass his circumstances, he sent for him, and said in a tone of affectionate reproach, "M. Berthollet, I have always 100,000 crowns at the service of my friends," and in fact this sum was immediately presented to him. Besides this, he was, upon his return from Egypt, nominated a senator by the First Consul, and afterwards received the distinction of Grand Officer of the Legion of Honour, Grand Cross of the Order of Reunion, Titulary of the Senatorerie of Montpellier; and under the empire, he was created a Peer of France, receiving the dignity of Count. The advancement to these offices produced no change in the manners of Berthollet; of which he gave a striking proof by adopting as his armorial distinction, at the time when others eagerly blazoned

some exploit, the plain unadorned figure of his faithful and affectionate dog. He was no courtier before he received these honours, and he remained equally simple and unassuming, and not less devoted to science, after they were conferred.

It was in 1803 that Berthollet published his work on Chemical Statics. Lavoisier had established almost nothing positive or precise with regard to chemical affinity, and it remained a topic which few cared to assay, on account of its difficulty, until the researches of Berthollet, at the Natron Lakes of Egypt, suggested to him a train of new ideas on the subject which were now published under the appropriate title of the *Statique Chimique*.

Chemists had no sooner made themselves familiar with the distinctive characters by which individual substances may be recognised, than the unequal energy with which two bodies act upon a third became a matter of notoriety. Geoffroy, one of the earliest of those who took a philosophical view of the nature of combination, advanced a general theory on the subject, which was eagerly embraced by the chemical world, and which his successors, particularly Bergmann, contributed materially to extend and to complete. According to this theory, chemical affinity, or the reciprocal tendency of substances to combination, is an invariable force: its intensity also is different in each individual substance, and is expressible in numbers. This second property was described in other words by saying that affinity is *elective*: that is, a substance already combined with another, when presented to a third, for which it possesses a still more energetic affinity, separates from the former, and, by preference, attaches itself exclusively to the latter.

This hypothesis of the existence of an infinite number of forces, all varying in their intensity, appeared to Berthollet inconsistent with the ordinary simplicity of nature; and the *Statique Chimique* was an attempt to demonstrate that, just as under the same law of matter we see a stone fall to the earth, and smoke rise from its surface, so the most opposite chemical phenomena are deducible from the existence of a single active principle, variously modified in its effects by a very few other causes, which, like it, are equally distinct and unalterable.

Chemical affinity he regarded as a force, analogous in all its effects, and probably indeed identical with the attraction of gravitation. Like the latter, its invariable tendency is to produce combination, and its intensity is proportional to the quantity of the body in which it acts. But although it is probable that these two forces are ultimately of the same nature, there is an important difference in the manner in which they are exerted. Gravitation consists in the mutual attraction between two masses of matter, situated at sensible distances from one another. Its effects are, therefore, dependent exclusively on

the *quantity* of matter existing in each mass ; and as the law by which the attractive force diminishes with the distance is accurately known, they may, in every instance, be subjected to the most rigorous calculation.

Chemical affinity, on the contrary, consists in the reciprocal attraction between the ultimate particles of substances, between which there intervenes only an insensible distance. In these circumstances, the mere attractive force, instead of acting undisturbed, is modified by the peculiar affections of the molecules, as by their figure, their distance, &c.; and as these affections undoubtedly vary in the ultimate particles of every different substance, it is obvious that the modifications which may thus be produced upon the attractive force are infinite. And this is the reason why it is impossible to estimate beforehand the amount of attraction which will take place between the molecules of any two substances : it must be investigated experimentally.

If this doctrine be correct, the assumption made by Bergmann and his predecessors, of an infinite number of distinct forces, all varying in their intensity, was gratuitous, and altogether unnecessary : they are all the results of one great power, modified more or less by the peculiar qualities of the substances in which it acts.

An example may serve to contrast in a still more striking manner the opposite views respecting chemical combination to which these two theories conduct. Suppose that to a mixture of two acids we add a quantity of an alkali insufficient to neutralize any one of them separately ; what will be the result ? According to the theory of elective attraction, the alkali will attach itself exclusively to the acid for which it possesses the most powerful affinity ; while the whole of the other acid, and that portion of the stronger acid in excess over what is necessary to produce exact neutralization of the alkali, will remain in a disengaged state in the liquid. Berthollet, on the contrary, maintained, that as the constant effect of attraction is combination, and as the degree of attraction is proportional to the mass of the attracting body, the two acids will share the alkali between them, and the amount of alkali with which each will be combined, will be in the compound proportion of its quantity and the intensity of its attractive force.

At first view, this theory may appear to leave unaccounted for the decompositions which are such frequent consequences of chemical action, and which are so admirably explained by Bergmann's principle of election. If, for example, to a solution of acetate of lime we add oxalic acid, the whole of the lime will be precipitated in the state of oxalate, and the supernatant liquid will contain uncombined acetic acid. If the constant effect of affinity be combination, it may be asked, what is the reason that in this instance the whole of the lime does not remain in solution ;

and combine with both acids, in the ordinary compound ratio of their affinity and quantity?

Berthollet's explanation of this seeming anomaly was exceedingly luminous, and indeed constitutes the leading characteristic of his theory. The force, said he, which produces combination among substances whose constitution is different, and which is usually styled *Chemical Affinity*, is merely one of the effects or modes of action of the general principle of molecular attraction: another, no less extensive and powerful, is the influence of this principle in producing combination between particles of a similar constitution. This latter force, we have been accustomed to call *Cohesion*; and it has been too frequently regarded as a power *sui generis*, as a physical in contradistinction to a chemical power of matter, and one which is annihilated the instant it is overcome. On the contrary, as is the case with every other compressed natural force, it continues to act even after, by the intervention of some more powerful principle, the particles of the homogeneous solid have been completely disunited.

Hence in every case of chemical combination and decomposition, the affinity of cohesion and the affinity of combination must constitute direct antagonists to one another's action; and when two substances are placed in a situation favourable to chemical action, they will either remain unaltered or a combination will take place, according as either of these two forces predominates in intensity over the other. According to this theory it should be observed, decomposition must be proscribed from the list of the causes which tend to produce chemical changes: they are invariably the consequences of combination. Thus, in one of the illustrations already adduced, the cohesive affinity of the constituents of the oxalate of lime, is more than sufficient to counterbalance both its tendency to combine with water, and the affinity of the acetic acid for the lime: it consequently precipitates, and leaves the acetic acid in a disengaged state in the liquid.

Another important circumstance which modifies the affinity of combination is elasticity. Many substances acquire such a tendency to expansion by combining with caloric (which is the cause of expansibility), that they become no longer obedient either to the affinity of combination or of cohesion: whenever, therefore, their expansibility is sufficiently augmented by the accumulation of heat, or when the affinity by which they were held in combination is weakened by the intervention of a third body, they quit the solid or liquid in which they had previously existed condensed, and assume the form of an elastic gas or vapour.

Such is a very general outline of Berthollet's theory of affinity; but it would be impossible within the limits to which we are necessarily restricted, to convey an adequate conception

of the profound reasoning and ingenious experiments by which he endeavoured to estimate the exact amount of affinity exerted in chemical combination, and the extent to which it is modified or counteracted by its several rival forces, as by cohesion, by expansion, by heat, sometimes favouring combination by diminishing cohesion, sometimes opposing it by rendering one of the substances elastic, by light, by atmospheric pressure, and by the slow in opposition to the rapid propagation of chemical action.

The recently established principle of the equimultiple proportions in which substances combine, has contributed materially to weaken the confidence with which chemists were disposed to receive the conclusions of Berthollet; and indeed it is undeniable that many of his assumptions were too vague, and too little supported by experimental evidence, to be admitted unconditionally; that the principles by which he attempted to estimate the exact amount of affinity exerted in chemical combination were inconclusive; that he placed by far too high an estimate on the efficacy of mass in chemical action; and that he made a most unwarrantable generalisation when he transferred to the constitution of solid bodies those laws of affinity to which he had rendered it probable they are subject while in a state of solution. It is not too much to expect that the final establishment of an atomic theory, unembarrassed by arbitrary assumptions, taken in conjunction with the electro-chemical theory, will conduct to a still more perspicuous conception of the laws which regulate chemical combination; and it is probable too, that these views will be found to coincide with the opinions entertained by Berthollet to a much greater extent, than many chemists of the present day appear disposed to admit.

The publication of the *Statique Chimique* involved Berthollet soon afterwards in his celebrated discussion with Proust respecting the proportions in which substances enter into combination. This contest, in which the two most distinguished chemists in Europe took diametrically opposite views of what may be said to form the very basis of their favourite science, could not fail to excite a deep sensation from its commencement; and the extent of information possessed by each, and the admirable ingenuity with which each availed himself of his resources, all contributed to render the controversy more and more interesting as it proceeded. Perhaps it is not going too far to assert, that since Bergmann's enforcing the use of the balance in chemical investigations, nothing has contributed so much to the establishment of the doctrine of chemical equivalents as the views respecting combination which Proust on this occasion promulgated and supported. As this great doctrine has rescued chemistry from the domain of empiricism and uncertainty, and has elevated it to the rank of a mathematical science, and as its ultimate establishment is owing more than is generally acknowledged to

Proust's reasonings in this very controversy, which is on so many other accounts interesting, I shall make no apology for presenting an outline of it to the reader.

It is a matter extremely obvious to observation, that there are certain proportions in which chemical substances combine by preference with each other. Thus from all the various processes by which it is possible to unite oxygen with iron, the result (with the exception of a single more recently discovered oxide) is constantly *one of two* combinations, the proportions of which are unalterable. These are familiar in chemistry as the *black* and the *red* oxides of iron; each of them characterises and forms the basis of a peculiar class of salts; and no other oxide of iron has a name, or is known. The general truth of a combination by preference, it was impossible for Berthollet to deny; but he affirmed that wherever two bodies possess a reciprocal affinity for each other, they *may* combine in an infinite variety of proportions. He accounted for the apparent preference by supposing that wherever it exists, it is a consequence of the interference of some foreign principle, such as cohesion, elasticity, &c. with the simple operation of affinity. Thus, he alleged, when any ordinary combustible, as hydrogen or sulphur, or any volatile metal, as zinc or arsenic, is ignited, it is converted into vapour by the elevated temperature, its cohesive attraction is overpowered, it is at once placed in the situation the most favourable for combining to saturation with oxygen, and the result is of course an instantaneous absorption on their part of the greatest portion of that air with which this process can ever unite them. But, on the other hand, said he, when any of the more fixed metals is ignited, as tin or lead, it undergoes a process of progressive combination by imperceptible degrees of increase with oxygen, forming compounds of every variety between zero and that dose which constitutes the saturated oxide.

The arguments of Berthollet were affirmed by his antagonist to be wholly without foundation, and his experiments were pronounced to be either inaccurate or inconclusive. With respect to the native oxides of iron, the instance quoted by Berthollet which seemed the most forcibly to illustrate and support the view of combination in infinite variety of proportion, Proust took a true, and, at the same time, a most ingenious view. He conceived that although a given mass of *oxidized iron* may be found, the constituents of which are resolvable into oxygen and the metal in any proportions between the maxima and minima in which these substances are ever found united, yet in every such case the mass is composed of the black and the red oxides, mixed through each other in every various proportion, and still no atom of oxygen is combined with any one atom of iron unless in one or other of the proportions which make the black or the red oxide. And the mode in which he proved all oxidized iron

to be composed of the black and red oxides only was simple and decisive. The red oxide of iron has a less affinity for acids than the black. Take any oxide of iron supposed to be intermediate between these two, take any pretended *third* oxide, and to a warm solution of it in muriatic acid add potash in small quantities at a time. At first, the precipitate obtained is of a red colour, and consists of the pure *red* oxide: by and bye a change almost instantaneous takes place in the colour of the precipitate, which now becomes *green*, and is neither more nor less than a hydrated *black* oxide. This latter oxide continues to be the precipitate obtained by this process, so long as an atom of iron remains in solution. Every pretended new oxide subjected to this treatment is thus resolved into two, the black and the red, nor is it possible to detect during the slowest process of precipitation a single vestige of any intermediate separated oxide. Of course the fair conclusion is, that no such third oxide existed in the solution, and thus the proof that there is no such third oxide is nearly as conclusive as any proof of a *negative position* ever can be.

Perhaps the true secret of the question being hung up in suspense so long as Berthollet argued on the other side, is to be found in the fact that Proust had to establish a negative position. If Berthollet could discover any *one* substance supporting his doctrines, his case was made out; while Proust, to give even feasibility to the views he adopted, was obliged to solve every appearance quoted by his antagonist. Under such circumstances it is plain that a man of the extensive knowledge and penetrating ingenuity of Berthollet could not fail to force an opponent to a very wide range of investigation ere he could hope to establish his theory. Accordingly, besides the oxidized masses of iron, it was necessary for Proust to examine the imperfect oxides of lead, copper, arsenic, tin, &c. and to resolve *all* of them into those well-known perfect oxides which he alleged to be their invariable state of combination. In this difficult task he was eminently successful, and as an interesting example of his mode of proceeding, we shall quote his experiments on the calcination of tin, and the results which so fairly flowed from them. Take a mass of tin oxidized by calcination, and wash it in water: it immediately discovers itself to be a mixture of unchanged metal and oxidized tin; for the former, being much heavier than the latter, is disengaged from it in minute particles during this operation, and in fact, in the original mass, ~~this~~ pure metal was merely enveloped in an external coating of the oxide of tin. Take the oxide of tin thus obtained in a state comparatively genuine, and introduce it into cold muriatic acid: a large portion of it passes into solution, and an additional residue of metallic particles is still obtained, which, on account of their tenuity, had been carried along with the oxide during the wash-

ing. Since, therefore, in this mass of oxidized tin, much of the metal had remained uncalcined, the solution, conformably to Berthollet's theory, ought to contain an oxide of tin at a minimum, or at least at one of the infinitely numerous inferior degrees of oxidation. But so far is this from being the case, that the solution when examined with reagents is found to hold a muriate of tin at exactly a *maximum* of oxidation. The slow calcination of tin, therefore, does not afford the slightest evidence of an ascending oxidation.

From the results of this experiment, and from the investigation of other imperfect calcinations, Proust's theory was placed on a tolerably broad foundation, and he soon gained another advantage by an experiment of Berthollet's, which, had it succeeded as the latter had expected, must have established his theory, but which proved so untractable as to involve him in no small difficulty, and from which it required all his ingenuity to extricate himself with any eclat. Berthollet took a solution of nitrate of mercury, in which he presumed the acid might be obtained combined with the metal in every stage of its oxidation between the maxima and minima proportions. He very reasonably inferred, therefore, that by adding muriate of soda to the solution, a variety of analogous compounds might be formed of muriatic acid with these various oxides of mercury. He made the experiment with every caution, but his *external or contingent principles* constantly interfered with the pure operation of affinity, for the results were only two compounds, calomel and corrosive sublimate. Both of these are well-defined and invariable combinations of oxygen and mercury with muriatic acid (to use the chemical language of that period), the first being an union of the acid with the metal at a minimum; the second of the acid with the metal at a maximum of oxidation. Proust's explanation of this apparent anomaly was a plain and obvious one. All solutions of the nitrate of mercury must consist, besides the acid, of the metal at a maximum of oxidation, or at a minimum of oxidation, or finally of a mixture of these oxides. In decomposing these solutions with muriate of soda, the product of the first will be muriate of oxide of mercury at a maximum of oxidation, or corrosive sublimate; of the second, the product will be muriate of the oxide of mercury at a minimum of oxidation, or calomel; and the products of the third will be a mixture of the two substances just mentioned, or both corrosive sublimate and calomel; in every possible proportion.

The only means by which Berthollet could account for these results was to suppose that the mercury assumes these two constant states of composition only at the instant when it is on the point of separating itself into two combinations, and that these two definite oxides are formed only at the very point of time when the muriatic acid decides their separation into a soluble

and insoluble salt. Alas! how often in theory, as well as in practice, does the problem become strangely difficult of making the two ends meet. How often do we find that on the insect wing of *one small moment* ride the eternal fates! Mercury is indeed a nimble, sprightly metal, yet one cannot help assenting to the observation of Proust, that in this theory there is really too much stress laid on both its agility and intelligence. Here, said he, we find this countless host of infinitely various and distinct oxides, all constituting separating nitrates, in an instant, and *per saltum*, as it were, abandon the stations they had occupied in the scale of their thousand and one oxidations, to fly to the extremes of that scale, which happen also to be the posts of calomel and corrosive sublimate, the only points at which they will suffer the anxious investigator to come up with and secure them! What promptitude! What exactness! Really, says Proust, one must concede at least to M. Berthollet, that nothing can surpass the admirable evolutions and discipline of *his* oxides!

It was in this manner that the controversy was concluded on both sides, in the most liberal spirit, and at the same time with the most lively argument and research. But before we take leave of it, it is no more than due to Proust, in order to show how admirably just and perspicuous was his exposition of the views which the doctrine of chemical equivalents unfolds, to state what he himself then wrote on the subject. Take an instance of his reasoning relative to the oxidation of metals.

The existence of an infinite number of distinct and independent oxides is inconsistent with the ordinary progress of nature in every thing else. In the oxidation of metals, nature follows the same course as in combining oxygen with any of the combustibles. The latter combine avowedly with definite and invariable proportions of oxygen; and the former, when placed in a situation favourable to their union with that substance, combine, at the instant of contact, with the whole quantity of it required to produce saturation at one or other of the points which in common terms we style the maximum or minimum of oxidation. In the same manner, when a molecule of any alkali is placed in contact with an acid, it does not at first combine with less and then with *more* of the quantity necessary for saturation: on the contrary, it *instantly* attracts the *whole* proportion of acid, with which, in obedience to the invariable laws of its affinities, it at once forms a complete combination. The proportions in which substances unite have been fixed by nature from all eternity, and are as little under our controul as are those affinities by which the compounds are upheld. *Election and proportion are two poles around which the whole system of true combinations invariably revolves, whether in external nature, or in the investigations of the chemist.* From their agency result the laws exerting that

unchangeable and universal dominion over matter, which produces the harmony between all the relations and properties of the compounds formed by nature, with those of the same combinations produced in the laboratory. This assigns those limits of saturation, which neither nature nor art can for an instant vary. And *there is*, says Proust, repeating the words of Berthollet, with which the latter had reproached him as implying an extravagant proposition, *there is indeed an equilibrium obedient to the decrees of nature, which determines, even in our laboratories, the proportions of every combination.*

Such is an outline of the profound views on the subject of combination entertained by Proust. We have also seen those supported by Berthollet, and the manner in which their controversy was conducted. Upon viewing the whole, we must admit that it is rare in history to meet with philosophers so eminently celebrated, maintaining doctrines so opposite, upon a subject so important, yet without for a moment stooping to any thing unfair, or permitting the smallest rancour to mingle in their discussion. It must indeed be admitted that he who had the most difficult part to support, does occasionally evade arguments which it was impossible fully and directly to meet; and also that his awkward manner of experimenting occasionally put it in his adversary's power to correct some of his positions. But we must still hold that the experimental and argumentative ingenuity of either party was well poised against the other, and that it would be difficult to point out such another scientific discussion in which there may be found so much to interest, while nothing occurs that can for a moment offend.

It was the prevalent idea previous to this period that the putrefaction which water always undergoes after being long kept in wooden casks, and which so greatly injures its taste and smell, is the effect of an inherent principle which accompanies the liquid from the spring. Berthollet, however, conceived the cause of this putrefaction to be *the solution of an extractive matter from the wood*, and that this might be prevented by charring the inside of the cask. This process would possess the double advantage of wholly excluding the water from the wood on the one hand, whilst the antiseptic qualities of the carbon must check any putrefactive tendency in the water whatever might be its origin on the other. He accordingly took two casks of the same materials, charred the interior of one, and filled both with water. At the end of four months, the water in the charred cask had contracted no unpleasant taste or smell whatever, while the water in the other was become so putrid, that its very smell was intolerable.

Not long afterwards, the celebrated navigator Krusenstern, having seen a statement of this mode of preserving water quoted in a periodical journal, immediately put it in practice with a

portion of his water casks, and after a few months' experience, he subjected them all to this process. The comfort which he enjoyed from it he mentions in a letter to a scientific friend, dated Kamschatka, July 8, 1805 :—"Our water," says he, "has been constantly as pure and good as that of the best spring. We shall thus have had the honour of being the first to put in practice a process so simple and so useful, and the French chemist will perhaps receive pleasure from learning the happy issue of the method he proposed."

It is a little harassing to be obliged to state now in 1825, that neither the simplicity, nor the manifest advantage of this system, has yet introduced it into general use. But it has been found of as much benefit to char the interior of casks in which wine is kept, as of those for containing water. Wine possesses also the property of dissolving an extractive matter from the wood, which injures its flavour, and peculiarly exposes it to the acetous fermentation. It is in consequence of this, that *well-seasoned* wine casks are much preferable to *new*; but, for the same reason, charred casks are much preferable to either. Berthollet himself was the first to suggest this application of his process, and at his request, M. Paris, an intelligent wine merchant, put his proposal to the test of experience. In a few years he wrote to inform M. Berthollet that the wine preserved in these casks was more rich and generous than it could have been under any other treatment. It is really difficult to say whether M. Berthollet is most to be admired for the profoundness and originality of his scientific views, or for his tact and felicity in applying discovery to useful practice.

As we advance towards the latter periods of the life of Berthollet, it is delightful to find, even under his silver hairs, the same ardent and unremitting zeal in the cause of science, which had glowed in his earliest youth, accompanied by the same generous warmth of heart that he had ever possessed, and which displayed itself in his many intimate friendships still subsisting, though now mellowed by the hand of time.

At this period, La Place, beyond comparison the profoundest astronomer and mathematician of his day, lived in or near Arcueil, a small village situated three or four miles from Paris. Between this great man and Berthollet, there had long subsisted a warm affection, founded on mutual esteem. In order therefore to be near each other, and enjoy the more frequent intercourse, the chemist purchased a country-seat in the village. Here he established a very complete laboratory, fit for conducting all kinds of experiments in every branch of natural philosophy; and there soon flocked around him a number of distinguished young philosophers, most of whom had been the pupils of Berthollet, and who knew that in his house their ardour would at once receive fresh impulse and direction from the example and coun-

sels of their former instructor; while at the same time they should be readily supplied with the means of conducting those experiments in which an expensive apparatus was requisite.

Among the most assiduous and successful of these young men was A. B. Berthollet, the son of the illustrious chemist. He had already rendered no small service to his countrymen, by the zeal and assiduity with which he had co-operated with his father in preparing and publishing a new and greatly improved edition of that valuable work, the *Elémens de l'Art de la Teinture*. The names of the father and son stand together on the title-page as joint authors, and the natural affection which must ever subsist between two persons connected by so intimate a degree of relationship was in their case strengthened and exalted by a community of feeling, and, by kindred pursuits. To the chemical world in general the younger Berthollet is well known, by his discussion with Proust respecting the constitution of hydrates and metallic oxides; by his memoir on ammonia, in which he combated successfully an opinion of Davy's, and established the general accuracy of his father's previous analysis; by his essays on the chloride of sulphur, and Lampadius's alcohol of sulphur.

Surrounded by a company of youthful philosophers like these, it occurred to Berthollet that their organisation into a Society would introduce a method and regularity into their researches, which, whilst it must be delightful to the individuals themselves, could not fail to advance materially the cause of science. This was the origin of the celebrated *Société d'Arcueil*, which unfortunately was as short lived as it was illustrious. M. Berthollet was himself the President, and the other original members were La Place, Biot, Gay-Lussac, Thenard, Collet-Descostils, Decandolle, Humboldt, and A. B. Berthollet. In this class, we find respectively the most distinguished men in astronomy, mechanical philosophy, chemistry, and botany, which France or Europe could boast of, and the traveller Humboldt, belonging to no class in particular, but whose profoundness equals the universality of his knowledge.

This Society in a few years published so many as three volumes of the most interesting memoirs, in which we find the President, notwithstanding his advanced age, still one of the most active and able contributors. It is worth while to make an extract from the Introduction to the first volume, in order the more clearly to show the plan and the design of the Institution. "There has been formed an association of a few persons, the votaries of natural philosophy in all its branches, with a view to the improvement of the powers of each individual, by the aid derived from an union of which the basis is mutual esteem and similarity of taste. It has been instituted with the addi-

tional design of escaping the inconveniences which attend too numerous an association." Next follows an account of its constitution, its meetings once a fortnight, the performing of new experiments, the presentation of memoirs, the discussion of their merits, the criticising the periodical journals of science, &c. After this we find a singularly beautiful and delicate passage from the hand of Berthollet, in which he calmly contemplates at once his own approaching dissolution, and the contrasted view of the eternal duration and progress of knowledge. To read it does honour to Berthollet: its sentiment reflects dignity on human nature. "He with whom originated the plan of this Society," says he, "now feels as he beholds the end of his career draw nigh, the sweet satisfaction of having thereby contributed far more effectually to the advancement of those sciences to which he has long devoted himself, than he could have done by those works which he may yet be able to carry on ere he die."

So calm a resignation to meet without regret the close of a life which had been so rich in fame to himself, and in benefit to his country, accompanied by so pure, so enthusiastic an attachment to science, it is highly delightful, and it is eminently useful to contemplate. After this it is severely painful to learn, that the energy of this Society was soon paralyzed by an event which embittered the latter days of the life of Berthollet, even then, when all seemed to promise it a quiet and a tranquil end. The promising son of Berthollet, in whom his happiness was wrapped up, was unhappily subject to the fearful malady of despondency, which at length grew upon him to such a degree, that neither the rank and fame of his father, nor the affection of his aged mother, nor the respect of friends, nor the honours which science seemed to hold out to his young years, could prevent it from gaining a gloomy mastery over his soul. He grew weary of his existence, and at length his life became wholly unsupportable. Retiring to a small room, he locked the door, closed up every chink and crevice which might admit the air, carried writing materials to a table, on which he placed a second watch, and then seated himself before it. He now marked precisely the hour, and lighted a brazier of charcoal beside him. He continued to note down the series of sensations he then experienced in succession, detailing the approach and the rapid progress of delirium, until, as time went on, the writing became confused and illegible, and the young victim dropped dead upon the floor!

After this event, the spirits of the old man never again rose, for the spring of his hope was broken, and the stay of his age was gone. Occasionally some discovery, extending the limits of his favourite science, engrossed his interest and attention for

a brief period; but such was the sole comfort he ever afterwards knew, and it too was rare and short-lived. The only work which he seems to have undertaken subsequent to this period is a memoir on the analysis of vegetable and animal principles, a field of investigation in which he had already distinguished himself, and in which, once more, with his usual profound penetration, he anticipated and led the way to the recent discoveries of Gay-Lussac and Thenard, who reduced these complicated combinations to their elements by means of combustion.

It was in this heart-broken manner that the remaining years of Berthollet were spent. It is indeed an awful lesson to the frailty of human nature to see a happiness the purest that man can ever enjoy, cut at once to the heart's core, to witness the near prospect of the tranquil close of so long a life, which in every vicissitude had been adorned by honour and integrity, and in many a period gloriously illuminated by fame, in one awful moment broken up and clouded for ever. From the day that his son died, no smile ever passed over his features; his air, once so sprightly and cheerful, remained sombre and gloomy; and often the unbidden tear forced itself down his aged cheek. Death seemed no longer an evil, as life seemed to separate him from his child. And in a few years, that stern but sure comforter reached the melancholy Berthollet.

His end was worthy of the manner in which he had lived. A fever, apparently slight, left behind it a number of boils, which were soon followed by a gangrenous ulcer of uncommon size. Under these he suffered for several months with the greatest constancy and fortitude. His complaint was of that desperate nature which medicine cannot cure. He himself, as a physician, knew the extent of his danger, felt the inevitable progress of the malady, and steadfastly but calmly regarded the slow advance of death. During all this time, his mental suffering, and the loss of his son, engrossed him more than his bodily pain. At length, after a tedious period of suffering, in which his equanimity had never once been shaken, Berthollet died on the 6th of November, 1822, at the advanced age of 74 years. He has left the faithful partner of his joys and griefs, to mourn his loss in desolate, childless widowhood.

The robust constitution of Berthollet had led his friends at one time to anticipate for him a much longer life. But the weight of grief which latterly oppressed him gave a fearful strength to the disease that invaded him, and these together seemed to cut him off ere his full time had yet arrived. Since the death of his friend Monge, and the illustrious La Grange, the sciences in France have not sustained so severe a loss. One of the founders, and always one of the best supporters and elucidators of modern chemistry, save only the gloom of his latter years,

no man ever ran a longer or a more brilliant career of fame. As a votary of science, who has done more than he who first explained the combination of oils with alkalies and metallic oxides; who crystallized the fixed alkalies, and gave them a pure causticity; who decomposed nitric acid and ammonia; who discovered fulminating silver; and who, the first great analyst of vegetable and animal substances, detecting the peculiar characteristics of each, has opened up a new and interesting branch of study to the chemist! As a philosopher and theorist, who is more distinguished than the first great chemist who acceded to the system of Lavoisier; who, for more than twenty years, gave to the scientific world the law on the important subject of the nature of chlorme, who was the first to see and maintain, in spite of prejudice, that oxygen is not the sole acidifying principle; who, again superior to the errors of his day, demonstrated that metallic oxides have an acid nature in one combination and an alkaline in another; and who, in fine, is at this moment the author of the profoundest work on chemical affinity which has as yet seen the light! If again we turn our eyes from the interests of the past and present to those of the future; if we divert our attention from the existing activity and research of the busy philosopher, to look to that careful forethought and watchfulness which already provided for the promotion of science during the progress of ages, of centuries after his material part should be resolved into its original dust; if we look for a man, who, remembering the shortness of life and the frailty of individual exertion, was anxious to organize those corporate scientific bodies, whose duration should terminate only with the end of the human race, and whose methodical advances in the career of knowledge should be regular and certain; who has done more for the time unknown to come than the assiduous Academician, the leading member at the formation of the National Institute of France, the founder of the Institute of Egypt, the affectionate father of the illustrious Society of Arcueil! Should we again regard the man of business and philanthropist, ever studious to advance the comforts of humanity, what man is a greater benefactor to his species than the author of the *Elements of the Art of Dyeing*; the instructor of the process of extracting soda from sea salt; the indicator of the mode of illumination by gas; the friend who supplies the unwearied seaman with a wholesome beverage in his lonely voyage; the man, in fine, whose name is identified with improvement itself, and ingrafted into his native tongue;—an eternal memorial of the benefits he conferred on one of the most common, the most useful, and the most universal of the arts!

When, in addition to all this, we find in Berthollet that zealous love of country, which made him the soul of the miraculous

energy of France, when she bounded forward at once over many eras of improvement, and took her rank with the first of modern civilized nations, joined to that store of knowledge which enabled him to give her in her desperate need that abundant supply of ammunition with which she repelled singly the assaults of an embattled world, we are apt to ask, what patriot had ever half the ardour, or half the happiness of Berthollet? He was, in fine, one of the most open-hearted, one of the most sincere, and kind of human beings. How beautiful a trait of simplicity of character is the first step that he makes into the great world—the first acquaintance that he forms in Paris—his unprepared approach to Tronchin, whose disinterested kindness seems to have been worthy of the open confidence reposed in him by the young physician, and who had thereby the honour to contribute most materially to the subsequent success of his protégé. Nor did any man possess more mild and unassuming manners than Berthollet, who, on one occasion, after a previous keen controversy, had rendered a certain philosopher almost afraid to meet him, nevertheless gave that very man so unreserved and so kindly a reception when they next encountered, as to force the tears of surprise and gratitude from his eyes.

Indeed no man ever had more friends, or preserved more sincere and lasting intimacies than Berthollet. He won men by his openness and candour, and he retained them by his affection and kindness. After all the honours to which he had been advanced, his deportment remained as simple and as unaffected as ever. He was never a courtier; and had the singular merit of being alike firm in his integrity under the reign of terror, and under both the reproaches and the favour of Napoleon. Alas! it is painful to think that a man every way fitted to adorn and exalt human nature, after so noble and honourable a course of life spent amid every danger and vicissitude, should have his latter days clouded by a fate so severe, yet over which he had no controul. But the sun shines, and the rain descends, alike upon the evil and upon the good. The sufferings are now gone,—the sorrows are now passed away,—but immortal among all who love science, country, or mankind, will be the hallowed memory of Claude-Louis Berthollet!

The following list will be found to contain nearly the whole of the works published by Berthollet:—

*Experiments on Tartaric Acid.* Journal de Physique, vii. 130.  
*Observations on Air.*

*Memoir on the Combinations of Oils with the Earths, the Vola-*

*tile Alkali, and Metallic Substances.* Mémoires de l'Académie Royale des Sciences. 1780. p. 1.

*Observations on the Phosphoric Acid of Urine.* Ibid. p. 10.

*Researches on the Nature of Animal Substances, and on their Relations with Vegetable Substances.* Ibid. p. 120; and, 1785, p. 331.

*Observations on the Combination of the Fixed Alkali with the Cretaceous Acid.* Ibid. p. 125.

*Essay on the Causticity of the Metallic Satts.* Ibid. p. 448.

*On the Analysis of Nitric Acid.* Ibid. 1781.

*Experiments on Sulphurous Acid.* Ibid. 1782, p. 597. Annales de Chimie, ii. 54.

*Researches on the Augmentation of Weight acquired by Sulphur, Phosphorus, and Arsenic, when they are converted into Acids.* Mém. de l'Acad. 1782, p. 502.

*Observations on the spontaneous Decomposition of certain Vegetable Acids.* Ibid. p. 608.

*Observations on the Causticity of Alkalies and of Lime.* Ibid. p. 616.

*Memoir on the Difference between Radical Vinegar and Acetous Acid.* Ibid. 1783, p. 403.

*Memoir on the Preparation of Caustic Alkali, its Crystallization, and its Action on Spirit of Wine.* Ibid. p. 408.

*Memoir on Dephlogisticated Marine Acid.* Ibid. 1785, p. 276.

*Observations on Aqua Regia, and on certain Affinities of the Marine Acid.* Ibid. p. 296.

*Memoir on the Decomposition of Spirit of Wine and of Ether by Means of Vital Air.* Ibid. p. 308.

*Analysis of the Volatile Alkali.* Ibid. p. 316.

*Observations on the Combination of Vital Air with the Oils.* Ibid. p. 327.

*Researches on the Nature of Animal Substances, and on their Relation with Vegetable Substances; or, Researches on the Acid of Sugar.* Journ. de Phys. xxviii. 88.

*Observations on the Comparative Analysis of Animal and Vegetable Substances.* Ibid. p. 272.

*On the Influence of Light.* Ibid. xxix. 81.

*On the Decomposition of Water.* Ibid. p. 138.

*Memoir on Iron, considered in its different Metallic States.* By Vandermonde, Berthollet, and Monge. Mém. de l'Acad. 1786, p. 132.

*Notes on the Analysis of a Green Cupreous Sand from Peru.* Ibid. p. 474.

*Memoir on Prussic Acid.* Ibid. 1787, p. 148.

*Observations on the Combination of Metallic Oxides with the Alkalies and Lime.* Ibid. 1788, p. 728.

*A Process for rendering the Oxide of Silver fulminating.* Journ. de Phys. xxxii. 474.

*Observations on some Combinations of Dephlogisticated Marine Acid, or of Oxygenized Muriatic Acid.* Ibid. xxxiii. 217.

*Notes on Kirwan's Essay on Phlogiston.*

*Outlines of a Theory of the Nature of Steel, and of its Preparations.*

*Description of the Bleaching of Cloth and of Thread by the Oxygenized Muriatic Acid, and of certain other Applications of that Liquor to the Arts.* Annales de Chimie, ii. 151, vi. 204, vii. 244, xi.

*Consideration of Priestley's Experiments relative to the Composition of Air.* Ibid. on an Article in the new Dictionary of Chemistry, at Mr. Kier. Ibid. iii. 67.

*Collection of Observations on Madder.* Ibid. iv. 102.

*Memoir on the Action of Oxygenized Muriatic Acid on the Colouring Matters of Plants.* Ibid. vi. 210.

*Elements of the Art of Dyeing.* 1 vol. 8vo. 1791. A new edition, greatly improved and enlarged, 2 vols. 8vo. 1814.

*Observations on the new Dictionary of Chemistry of Mr. Kier.* Ann. de Chim. x. 131.

*Observations on some Facts which have been opposed to the Antiphlogistic Doctrine.* Ibid. xi. 3.

*On Gallic Acid.* Ibid. xii. 312.

*Observations on the Use of the Alkaline Prussiates, and of the Prussiate of Lime in Dyeing.* Ibid. xiii. 76.

*Description of the Bleaching of Cloth.*

*Observations on Sulphuretted Hydrogen.* Ibid. xxv. 238.

*Notice on an Acid separated from Animal Substances, or Zoonic Acid.* Ibid. vi. 86.

*Observations on Natron.* Journ. de Phys. li. 5.

*On the Bleaching of Cotton and Linen by Carthamus.* Mém. de l'Inst. d'Égypte.

*Observations on Natron.* Ibid.

*Observations on the Dyeing Properties of Ithenne.* By Berthollet and Descostils. Ibid.

*Eudiometrical Observations.* Ibid. And Ann. de Chim. xxxiv. 73.

*Remarks on the Memoir in which M. Girtanner examines whether Azote is a simple or a compound Body.* Ann. de Chim. xxxv. 23.

*On the Action of Sulphate of Iron on Nitrous Gas.* Ibid. xxxix. 3.

*On the Composition of Sulphuric Acid.* Ibid. xl. 166.

*Researches on the Laws of Affinity.*

*On the Hygrometric Water of Gases; on the Oxides of Carbon, &c.* Ann. de Chim. xlii. 282.

*Observations on Charcoal; and on the Carburetted Hydrogen Gases.* Mém. de l'Inst. Nat. vol. 4, part 1, p. 269, 319, 325.

*Essay on Chemical Statics.* 2 vols. 8vo.

*On Chemical Nomenclature.* Ann. de Ch. xlv. 316.

*On Varnishes.* Ibid. xlviii. 84.

*Reply to Schnaubert's Examination of his new Theory of Affinity.* Ibid. xlix. 5.

*Report on Humboldt and Gay-Lussac's Memoir on the Proportions of the Constituent Principles of the Atmosphere.* By Chap- tal and Berthollet. Ibid. liii. 239.

*Observations relative to different Memoirs of Proust, inserted in the 59th volume of the Journal de Physique.* Jour. de Physique, ix. 284 and 347.

*Third Continuation of Researches on the Laws of Affinity.* Mém. de l'Inst. Nat. vol. vii. part 1, p. 229.

*On the Preservation of Water during long Voyages, and on the Preservation of Wines and other Liquids, by charring the Interior of the Casks.* Ann. de Ch. lix. 96.

*Report of a Memoir on the Indigo Vats presented by M. Gar- riga.* By MM. Vauquelin, Gay-Lussac, and Berthollet. Mém. Pres. à l'Inst. Nat. ii. 634.

*On the Alteration which Air and Water produce upon Flesh.* Mém. de la Société d'Arcueil, i. 333.

*Introductory Dissertation to Riffault's Translation of the Third Edition of Dr. Thomson's System of Chemistry.*

*Observations of the late M. Rose on Carbonate of Soda, and Notice of the Labours of that Chemist.* Ann. de Ch. lxx. 316.

*Report on a Memoir presented by M. Curadon, entitled "Experiments on Sulphur, and on its Decomposition."* By Vauquelin and Berthollet. Ann. de Ch. lxxvii. 151.

*Observations on the Proportions of the Elements of certain Combinations.* Mém. de la Soc. d'Arcueil, ii. 42.

*New Observations on the Inflammable Gases, designated by the Names of Carburetted Hydrogen, and of Oxi-carburetted Hydrogen.* Ibid. ii. 68, and iii. 148.

*On the Heat produced by Percussion and Compression.* Ibid. ii. 440.

*On the Oriental Bezoars.* Ibid. ii. 448.

*On the Changes produced on Air by Respiration.* Ibid. ii. 454.

*On the reciprocal Mixture of Gases.* Ibid. ii. 463.

*On the Relations of Quantity in the Elements of Combinations.* Ibid. ii. 470.

*On the Azote separated from Charcoal by Heat.* Ibid. ii. 484.

*Notes on "Experiments and Observations relative to the new Principle of the Action of Affinity established by M. Berthollet,*

*with some Reflections on the Measure of Affinity in General.*" By M. C. H. Pfaff. Ann. de Ch. lxxvii. 288.

*Report on a Memoir of M. Curaudan, entitled "General Considerations on the Properties of Oxygenized Muriatic Gas."* By Chaptal, Vauquelin, and Berthollet. Ibid. lxxx. 54, 112.

*Report on a Memoir of M. Cluzel, on the Analysis of the Liquid Sulphur of Lampadius.* By Berthollet, Thenard, and Vauquelin. Ibid. lxxxiii. 252.

*Report on a Memoir of M. Bérard, respecting the Physical and Chemical Properties of the different Rays which compose the Solar Light.* By Berthollet, Chaptal, and Biot. Ibid. lxxxv. 309.

*Report on a Memoir of M. Dulong, on a new detonating Substance.* By Thenard and Berthollet. Ibid. lxxxvi. 37.

*Note on a Memoir of M. Lowitz, on the Method of sweetening Putrid Water by Means of Charcoal.* Ibid. xciii. 150.

*Note respecting a Memoir of MM. Colin and Robiquet, entitled "Researches on the Nature of the Oily Substance of the Dutch Chemists."* Ann. de Ch. et de Phys. i. 426.

*Considerations on Vegetable and Animal Analysis.* Mém. de la Soc. d'Arcueil, iii. 64.

*Observations on certain Mercurial Precipitates, and on those of Sulphate of Alumina.* Ibid. iii. 77.

*Experiments on the Proportions of the Elements of Nitric Acid.* Ibid. ii. 165.

*Observations on the Composition of Oxymuriatic Acid.* Ibid. iii. 171.

*Note on the Decomposition of Sulphate of Barytes, and of Subcarbonate of Lime by Potash.* Ibid. iii. 433.

*Note on the Composition of Oxymuriatic Acid.* Ibid. iii. 603.

## ARTICLE II.

*A Summary View of the Atomic Theory according to the Hypothesis adopted by M. Berzelius.* By J. G. Children, FR $\ddot{S}$ .

THE general adoption of the peculiar views of M. Berzelius respecting the atomic constitution of chemical compounds, native or artificial, by the chemists and mineralogists of the European continent, especially those of Germany and Sweden, renders an acquaintance with it almost indispensable to the English reader, since scarcely a single analysis is now published in the scientific journals and treatises of those countries, the results of which are not calculated, according to the data, and

denoted by the symbols, of the illustrious Professor of Stockholm.

A short account of his doctrine will, therefore, probably not be uninteresting to our readers, though, perhaps, they may think with us, that the simpler theory usually adopted in our own country answers every purpose equally well, and with greater facility, than the more complicated system of our continental neighbours. But our object is to explain, and not to criticise. We proceed, therefore, to the details; which are partly abstracted from Berzelius's *Essai sur la Theorie des Proportions Chimiques*, and his *Nouveau Systeme de Mineralogie*, and partly from the work lately published by M. Beaulant, entitled *Traité Elementaire de Minéralogie*.

Berzelius has candidly admitted in the introduction to his *Essai*, that he has indulged largely in conjecture respecting the latent causes which regulate a few facts, and the laws that determine them; and that he attaches no further importance to those conjectures than conjectures generally deserve. As to his theory, however, respecting chemical proportions, he speaks with more confidence. "I here confine myself within the circle of experiment; and the laws which I have endeavoured to establish are the general result of experience derived from that source."

From the moment that bodies were considered as formed of simple elements, it seems to have been generally assumed, that similar external characters and internal properties indicate similarity of composition both in the nature and proportion of the elements of which compound substances are formed. A German chemist of the name of Wenzel appears to have been the first who attempted to establish the truth of this assumption, by an experimental investigation of a phenomenon which had already excited attention, namely, that when two neutral salts mutually decompose each other, the resulting compounds are also neutral. He showed that the relative proportions of alkalis and earths which saturate a given quantity of the same acid, are the same for all other acids; if nitrate of lime, for instance, be decomposed by sulphate of potash, the nitrate of potash and sulphate of lime which result from their mutual action preserve their neutrality: for the quantity of potash which saturates a given quantity of nitric acid, is to the quantity of lime which saturates the same quantity of nitric acid as the quantity of potash is to that of the lime which saturates a given quantity of sulphuric acid. Bergmann also bestowed much attention on the development of chemical affinities, but we are principally indebted to Richter for the first positive indications of chemical proportions derived from numerous experiments. He examined the phenomenon noticed by Wenzel, and explained it in the same way;

he also showed that when one metal is precipitated from its solution by another, the neutrality of the liquid is not affected.

The introduction of the antiphlogistic system by Lavoisier in some measure interrupted the progress of the doctrine of chemical proportions; and shortly after its establishment, Berthollet, in his celebrated *Essai de Statique Chimique*, endeavoured to prove that the elements of bodies have certain fixed points, a maximum and a minimum, beyond which they are incapable of combining, but that *between* those limits they may unite in any proportion.

Proust combated this opinion, and demonstrated that the combinations which metals form with oxygen and with sulphur are in fixed and invariable proportions, and that the supposed intermediate compounds between the highest and lowest degrees of oxidation and sulphuration are, in fact, merely mixtures of two definite oxides, or sulphurets.

In 1789, Mr. Higgins published his "Comparative View of the Phlogistic and Antiphlogistic Theories," in which he "conceived that when gases combine in more than one proportion, all the proportions of the same element are equal; and he founded this idea on the corpuscular hypothesis, that bodies combine particle with particle, or one with two, or three, or a greater number of particles." \* He did not, however, follow the clue he had thus happily hit upon, but left it to the genius of Dalton, "apparently without the knowledge of what Mr. Higgins had written"† to resume the subject, and give it a more extended application. Dr. Thomson in 1807 published a sketch of Mr. Dalton's hypothesis, in the third edition of his *System of Chemistry* (vol. iii. p. 424), by permission of its sagacious author; and in the following year, Mr. Dalton produced the first volume of his "New System of Chemical Philosophy;" and two years after, the second.

Adopting views similar to those of Higgins, Dalton supposes that bodies are composed of atoms, and that to form the simplest or binary compounds, an atom of A unites to an atom of B. A ternary compound results from the union of 2 atoms of A, + 1 of B, or 2 B + A, &c. and generally the atom of one element may combine with 1, 2, 3, or more atoms of another, but not in any intermediate or fractional degree; and an atom of a compound body may in like manner unite to 1, 2, 3, or more *integral* atoms of another compound. This hypothesis was afterwards confirmed by numerous experiments, amongst the earliest and most important of which are those detailed by Dr. Thomson in his paper on oxalic acid, and in Dr. Wollaston's on super-acid and sub-acid salts. Both these interesting com-

\* Davy's *Elements of Chemical Philosophy*, p. 107.

† *Ibid.*

munications were read before the Royal Society in 1808, the former on the 14th of January, the latter on the 28th of the same month. Berzelius justly remarks, that this discovery may be considered without exaggeration as one of the greatest steps that chemistry has ever made towards perfection, and that the honour of the invention of the doctrine of *multiple proportions* is due to Dalton alone.

In 1806, MM. Gay-Lussac and Humboldt found that one volume of oxygen gas combines with two volumes of hydrogen gas to form water, and the first of these two philosophers some time after discovered that gaseous bodies in general combine according to certain fixed laws, and that one measure of one gas unites either with half a measure of another, or with 1, 2, 3, &c. measures; or, in other words, that gases either combine in equal volumes, or the volume of one gas is a multiple by a whole number of that of the other. M. Gay-Lussac published a valuable paper on this subject, entitled *Mémoire sur la Combinaison des Substances Gazeuses, les unes avec les autres*, in the second volume of the *Mémoires de la Société d'Arcueil*, in the year 1809.

If we substitute the word atom for that of volume, and imagine the substance to be in the solid instead of the gaseous state, Gay-Lussac's theory coincides exactly with Dalton's, and is a direct proof in confirmation of its truth.

In 1812, Sir Humphry Davy published his *Elements of Chemical Philosophy*,\* in the sixth section of the first division of which work he has given a general view of the theory of definite proportions, and in a variety of other places has shown its importance in calculating the results of chemical action; and since that period many of the first chemists in Europe have attentively studied the atomic theory, and have confirmed it, by a multitude of valuable experiments: amongst the most conspicuous in this field of inquiry are Thomson and Berzelius.

When we reflect on the cause of chemical proportions, the most probable idea that presents itself to our imagination is, that all bodies are composed of elementary particles or atoms, incapable of mechanical division;† and which unite together in such a manner, that an atom of one element combines with 1, 2, 3, &c. atoms of another element. With this simple postulate, to which the mind readily accedes, it is easy to explain all the phenomena of chemical proportions, especially those which we call multiple proportions. By the union of two or more elementary atoms, compound atoms are formed which are as incapable of mechanical division as the former, and these again unite together to form still more compounded atoms, and so on

\* Is it always to be a source of regret, that only the first part of that invaluable work has seen the light?

† On the subject of the limited divisibility of matter, see Dr. Wollaston's beautiful paper "On the Finite Extent of the Atmosphere."—(Phil. Trans. for 1822, p. 89.)

to the most compound. Berzelius divides these several combinations into separate orders—the simplest, or the atoms of the first order, are composed of simple elementary atoms, and are either inorganic or organic. The former contain only two elements, the latter always three at least. Compound atoms of the second order are produced by the union of compound atoms of the first order; those of the third by the union of atoms of the second order, &c. Thus sulphuric acid, potash, and water, are compound atoms of the first order, since they consist only of a base and oxygen. Sulphate of potash and sulphate of alumina are atoms of the second order, being formed by the union of compound atoms of the first order. Anhydrous alum, which is composed of the two last mentioned salts, belongs to the third order; and crystallized alum, which, besides those elements, also contains several atoms of water, is an instance of a compound of the fourth order.

But taking it for granted that bodies are composed of indivisible atoms, certain laws must regulate their combinations, in order to establish them in definite chemical proportions, and on which the constancy of those proportions must depend. The existence of such laws and the probable manner in which elementary atoms combine, is inferred from experiment, which teaches us that

1. *An atom of one element combines with one, two, three, or more atoms of another element.* This is of most frequent occurrence, so that in the greater number of compound atoms, one of the elements enters only as a single atom.

2. *Two atoms of one element combine with three atoms of another element.* This may happen whenever the quantity of oxygen in a protoxide is to that in the deutoxide as 1 :  $1\frac{1}{2}$ ; as is the case with iron. If the first oxide be composed of an atom of base united to an atom of oxygen, the second must contain two atoms of base united to three atoms of oxygen; for there can be no such thing as half an atom. This apparent anomaly may be reconciled in a different manner, by supposing that there may be another hitherto unknown oxide of iron, containing half the quantity of oxygen that is contained in the lowest of the two known oxides. According to this view, an atom of base in the unknown oxide is united to one atom of oxygen, and in the two known oxides to two and three atoms respectively. Dr. Thomson with Berzelius adopts the former solution.

In the compound atoms of the second order, the ratio of two atoms of one element to three of another is found less equivocally, though the instances are rare. Thus the hydrated red oxide of iron is composed of two atoms of oxide combined with three atoms of water; the subsulphate of copper contains two atoms of acid and three atoms of base. Unless we consider these as the true proportions of the elements of the above compounds, we must suppose the oxide of iron, as well as the

sulphuric acid, to contain six atoms of oxygen; but till new facts show the probability of that supposition, there is sufficient ground for considering the ratio to be that of 2 : 3. There is no reason for supposing that two atoms of one element combine with four, five, six, or a greater number of atoms of another element, but in the varied productions of the mineral kingdom, we find compounds somewhat different from those we can produce in our laboratories. Amongst the silicates, combinations, in which three compound atoms of the first order are united to four of the same order are frequent, as in laumontite, amphotene, &c.; but in our artificial productions, analogous cases are extremely rare.

As a general conclusion, we may assume, that in inorganic compounds the simple atoms combine in very limited proportions; the most common is that of one atom of one element with one or more atoms of another element, so that in most compounds, one of the elements may be represented by unity: the next most common proportion is that of two atoms of one element to three of another; and in the mineral kingdom, in the compound atoms of the third and fourth orders, we sometimes meet with three atoms of one body united to four atoms of another body.

There is another law which, according to Berzelius, regulates the combination of compound atoms of the first order; namely, that oxidated bodies always combine in such proportions that the number of atoms of oxygen in one of them is a multiple by a whole number of the number of atoms of oxygen in the other; and, in like manner, in the combinations of sulphuretted bodies, the sulphur in one is a multiple by a whole number of the sulphur in the other. Or, to express the law in general terms, compound atoms of the first order, having a common electro-negative element, combine in such proportion that the electro-negative element of one atom is always a multiple by a whole number of the electro-negative element of the other.

The only known exceptions to this law are the acids of phosphorus, nitrogen, and arsenic, which combine with other oxidated bodies in such proportion that the number of atoms of oxygen in the oxide is one or more *fifths* of the number of atoms of oxygen in the phosphoric, nitric, and arsenic acids, and one or two *thirds* of the same number in the phosphorous, nitrous, and arsenious.

When two salts, having a common acid but different bases, combine, the number of atoms of oxygen in one of the bases is a multiple by a whole number of the number of atoms of oxygen in the other; and consequently the acid in one of the salts is a multiple by a whole number of the acid in the other. In alum and feldspar, the number of atoms of oxygen in the alumina is triple the number of atoms of oxygen in the potash; and in like

manner the quantity of sulphuric acid and of silica, respectively combined with the alumina, is triple that combined with the potash. In the double tartrate of potash and soda, the two alkalies contain the same number of atoms of oxygen, and are consequently combined with the same number of atoms of tartaric acid.

When two salts, having a common base but different acids, combine, the number of atoms of oxygen of the portion of base combined with each of the acids is a multiple by a whole number of the number of atoms of oxygen in the portion of base combined with the other acid; or, the number of atoms of oxygen in one of the salts is a multiple by a whole number of the atoms of oxygen in the other. We have an instance of this species of combination in datholite, a compound of borate and silicate of lime, in which the lime is equally divided between the two acids; whilst in the blue carbonate of copper, the quantity of base combined with the carbonic acid is twice as great as that combined with the water.

In organic bodies, the compound atoms of the first order contain at least three elements, oxygen, hydrogen, and carbon; and their atoms are capable of combining in all sorts of proportions, so that neither of them can be taken as unity with reference to the rest. But when organic atoms of the first order combine with compound inorganic atoms of the first order, as when a vegetable acid combines with an oxide, they follow the same laws that regulate the combinations of the compound inorganic atoms, and the oxygen of the organic atom is a multiple, or sometimes a submultiple, by a whole number of the oxygen of the compound inorganic atom. Hence these combinations present the same phenomena of definite proportions as those of inorganic nature, and it is only in the formation of organic atoms of the first order that combination is possible in all sorts of proportions.

In the earliest experiments with the voltaic pile, it was found that the common salt, a solution of which was usually employed to moisten the pasteboard discs interposed between each pair of metallic discs, became decomposed by its action; and about the year 1805, MM. Hisinger and Berzelius published their experiments on the galvanic decomposition of the muriates of ammonia and lime, and several other neutro-saline solutions.

The results of those experiments demonstrated that when electricity traverses a saline solution, or any other liquid conductor, its elements are separated in such a manner that some collect round the positive, and others round the negative pole; and that oxygen, acids, and oxidated bodies, terminate to the former, whilst combustible bodies, alkalies, and the earths find their point of rest at the latter.

On the 20th of November, 1806, Sir Humphry (then Mr.)

Davy, read his Bakerian Lecture, on some Chemical Agencies of Electricity, before the Royal Society, which was afterwards published in the Philosophical Transactions for the following year. In this celebrated paper, which obtained the prize offered by Napoleon Buonaparte for the best Essay on Voltaic Electricity, Sir Humphry Davy clearly promulgated, for the first time, the laws by which the chemical agencies of electricity are regulated, and the principles on which their powers of suspending or destroying the usual order of chemical affinities depends. He showed that different elementary substances have different electrical energies, some being naturally positive, and others negative, with respect to each other; and that when a compound, formed of two such elements, is decomposed by the voltaic battery, the body possessing positive energy is repelled by positively electrified surfaces, and attracted by negatively electrical surfaces; and that the body possessing the negative energy follows the contrary order.

Adopting these views, Berzelius divides all substances into two great classes, the electro-positive and electro-negative. Simple bodies belonging to the former class, as well as their oxides, always assume the positive state when they meet with other simple bodies or their oxides belonging to the latter; and the oxides of the first class bear the same relation to those of the second, that salifiable bases bear to acids. He considers oxygen as the most electro-negative of all bodies, and the only one whose electrical relations are invariable; it never being positive with respect to any other, and he places it accordingly at the head of his table exhibiting the supposed order of elementary substances with respect to their electrical relations. The last substance in the table, and consequently the most positive, is potassium, and all the intermediate substances between oxygen and that body are considered as negative to all those which stand below them, and positive to all that stand above them in the table.

Long before any idea had been formed of the electrical relations of simple combustible bodies, their oxides were divided into acids and bases, the first forming the electro-negative, the second the electro-positive class; amongst the bodies of the first class a weak acid often serves as base to a more powerful acid, and in the electro-positive series, a weak base frequently acts as an acid with respect to one more strongly electro-positive.

The electrical relations of oxides usually depend on those of their bases; thus an oxide is electro-negative to another oxide, if the base of the former be negative with respect to that of the latter, and *vice versâ*. Sulphuric acid, for instance, is electro-negative with respect to all the metallic oxides, because sulphur is negative with respect to all the metals: the oxides of potas-

sium and zinc, on the contrary, are electro-positive in regard to all oxidated bodies, with respect to whose bases potassium and zinc are positive. Hence acidity does not depend, as the antiphlogistic theory assumes, on oxygen as the acidifying principle, but rather resides in the radical of the acid, and the oxygen enters indifferently into the most electro-positive and electro-negative bodies, or the strongest bases and acids.

When the two electricities are separately manifested in any body, they are concentrated in two or more opposite points or poles, analogous to the poles of a magnetic needle. This electrical polarity, of which the tourmaline furnishes a striking instance, must also belong to the minutest particles of the body, as well as to the whole mass, and this affords an easy solution for all the phenomena of electro-chemical affinity; for the different forces with which elementary bodies unite may be conceived to depend on the different intensities of the electrical polarities of their atoms. This hypothesis, however, is not sufficient to explain why some bodies are *electro-positive*, and others *electro-negative*; but if we imagine the atoms of such bodies to possess *unipolarity*, similar to that which Ehrman observed in various substances, we may conceive that in the atoms of electro-positive bodies, the electricity of the positive pole predominates, and in those of electro-negative bodies, that of the negative pole, and consequently these bodies will always be in opposite states with respect to each other.

The degree of affinity between different bodies cannot however depend wholly on their specific unipolarities, but rather on the intensity of the polarities generally; for oxygen and sulphur, both electro-negative substances, combine with much greater energy than oxygen and copper, although the latter is an electro-positive element; and the influence of temperature has a powerful effect in modifying chemical attractions.

"If these conjectures," says Berzelius, "present a correct idea of the relations of bodies to electricity, it follows that what we call chemical affinity is nothing else than the effect of the electrical polarity of their atoms, and that electricity is the primary cause of all chemical action."

But it is time to quit this part of the subject, and turn to that which is more immediately the object of this abstract; and first we shall endeavour to explain the manner in which the relative weights of the atoms of bodies are determined.

(To be continued)

## ARTICLE III.

*Results of a Meteorological Register kept at New Malton, Yorkshire, in 1824. By Mr. James Stockton.*

Latitude  $54^{\circ} 8' 3''$ . Longitude  $0^{\circ} 47' 4''$  W. Height of the Cistern of the Mountain Barometer above the Level of the Sea, 92 feet.  
The Funnel of the Rain-Gauge is 7 feet from the ground.

1824. Months.	BAROMETER. Mean.	THERMOMETER.		WEATHER.		RAIN. Inches, &c.	CHARACTER OF EACH MONTH.
		Mean.	Range.	Wet.	Snow. Hail.		
January ...	30.015	36.025	27°	5	1	1.16	Calm, cloudy, and dry.
February ...	29.744	36.448	25	5	1	1.22	Idem.
March ...	29.704	38.516	34	7	6	2.83	Cold, wet, and changeable.
April ...	29.832	45.133	35	6	3	2.11	Changeable.
May ...	29.830	51.080	42	10	1	2.27	Cold and wet.
June ...	29.750	56.400	36	10	—	2.72	Fine to the 14th, then wet.
July ...	29.931	62.435	38	5	—	1.17	Warm and dry.
August ...	29.880	59.500	26	11	—	2.85	Idem.
September ...	29.607	56.066	54	11	1	5.61	Wet, windy, and changeable.
October ...	29.550	47.404	36	15	1	6.25	Excessively wet and stormy.
November ...	29.445	42.483	34	20	—	4.38	Excessively windy and wet.
December ...	29.656	38.706	32	12	3	4.17	Wet and changeable.
Annual means, &c.	29.710	47.683	64	117	15 5	36.74	

## ANNUAL RESULTS.

*Barometer.*

	Inches.
Highest observation, Jan. 16. Wind, NW. ....	30·780°
Lowest ditto, Nov. 23. Wind, SE. ....	28·210
Range of the mercury. ....	2·570
Mean annual barometrical pressure ....	29·770
Greatest range of the mercury, in January ....	2·210
Least ditto in May. ....	0·940
Mean monthly range of ditto. ....	1·349
Spaces described by its different oscillations ....	79·070
Total number of changes in the year ....	162·000

*Sia's Thermometer.*

Greatest observation, July 14. Wind, SE. ....	86·000
Least ditto, March 3. Wind, N. ....	22·000
Range of the mercury in the thermometer ....	64·000
Mean annual temperature. ....	47·683
Greatest range in September. ....	54·000
Least ditto in February ....	25·000
Mean monthly ditto. ....	34·916

*Winds.*

	Days.
North. ....	58
North-east. ....	52
East. ....	13
South-east. ....	19
South. ....	38
South-west. ....	70
West. ....	59
North-west. ....	31
Variable. ....	26
Brisk. ....	42
Boisterous. ....	39

*Rain, &c.*

	Inches, &c.
Greatest quantity, in October. ....	6·25
Least ditto, in January and July. ....	1·16+
Total amount for the year ....	36·74
Days of rain. ....	117·00
Snow. ....	15·00
Hail. ....	5·00

## REMARKS.

The mean temperature of the year just elapsed very nearly corresponds with that of 1819, and the amount of rain, which is about six inches less than in the preceding year, is on a similar par with that for 1822. Upwards of 20 inches, it will be observed, have fallen since the 1st of September, two-thirds of which fell by night, and frequently attended with most boisterous gales.

J. S.

*New Malton, Jan. 3, 1825.*

## ARTICLE IV. \*

*Facts respecting the Boiling Point of Ether.* By J. Bostock, MD. FRS. &c.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

*Upper Bedford-place, Feb. 12, 1825.*

THE following facts which I have observed respecting the boiling of ether, have, I believe, not been before noticed. If you think them of sufficient importance, they are much at your service for insertion in the *Annals*.

During the months of December and January, I was making some experiments on the action of ether and water upon each other, and particularly with regard to the effect produced upon ether by washing it. Among other circumstances I wished to ascertain the exact boiling point of ether before and after the operation of washing, and to compare this with the diminution of specific gravity which it experiences by this process. By heating ether of the specific gravity of  $\cdot 755$  in a matrass which contained a thermometer, over a spirit-lamp, I found that ascending and descending currents began to be visible in the fluid at  $107^{\circ}$ ; at  $110^{\circ}$  a few small single bubbles were formed, and that at  $112^{\circ}$  the ebullition was complete. It seemed, however, difficult to ascertain the object in view by this process, as a difference of  $2^{\circ}$  generally exists between the first formation of a single bubble and the production of what may be called complete ebullition. Besides it was often difficult to observe the exact height of a delicate thermometer, on account of the sudden bursts of vapour which arose from the fluid, and it occasionally happened that after ebullition had appeared to be going on at a certain temperature, it would cease, and not recommence until the thermometer had risen, perhaps, more than a degree. It appeared, however, that ether of the specific gravity of  $\cdot 755$  could not be raised above the  $112^{\text{th}}$  degree, and that at this point it was always in a state of complete ebullition.

In order to obviate these difficulties, I poured a quantity of ether into a wide test tube, and plunged the tube into a large jar of tepid water, the temperature of which was gradually raised by adding portions of hot water; I began at  $110^{\circ}$ , and was much surprised to find that it was not until the water had arrived at the  $150^{\text{th}}$  degree that the ether began to boil. Suspecting some peculiarity in the tube, I employed a second and a third with the same result; but upon trying a fourth, I observed a minute stream of bubbles rising up from one point of the glass, and on examining the part, I perceived a small fragment of some substance adhering to it. This occurrence led me to try the effect of introducing an extraneous body into the ether, and I accordingly dropped into it some small chips of a cedar pencil, which happened to lie on the table, when the wood was instantly covered with bubbles, and the fluid was quickly brought into a state of rapid ebullition, the bubbles appearing to arise, at least in a great measure, from the surface of the wood. Precisely the same effect was produced by portions of quill or filaments of feather.

I now reversed the experiment, and continued to add portions of cold water to the jar, in order to observe at what temperature the ether would cease to boil, when I found that the same ether, which scarcely boiled in a clean tube at  $150^{\circ}$ , by adding the pieces of cedar, exhibited a perceptible, although slight ebullition, when the water was at  $102^{\circ}$ . When the wood was first introduced, it was suspended in the upper part of the fluid, and was covered with a stratum of fine bubbles; by degrees, however, it appeared to be completely soaked in the fluid, gradually sunk to the bottom, and the ebullition nearly ceased; but by the introduction of a fresh piece, it was reproduced, and might in this way be continued at pleasure. Other substances were afterwards dropped into the ether; small fragments of broken glass lowered the boiling point very considerably, but not to an equal degree. When a small bit of metallic wire was dropped into ether at  $145^{\circ}$ , a sudden and copious explosion of gas or vapour was produced, and the ebullition afterwards continued at a much lower temperature, but the effect was so rapid and violent, that I could not mark the exact number of degrees of the depression; very nearly the same effect was produced by dropping copper filings into the ether, or immersing a thermometer. When the ether in the clean tube was plunged into the hot water, it assumed a waved or streaked appearance from rapid currents which were moving up and down it in various directions; and the process of evaporation went on so rapidly, that a very sensible degree of cold was experienced by the finger when held over the mouth of the tube.

Although in most cases the ether in the clean tube began to boil at about  $150^{\circ}$ , in some cases the water in the jar has been

raised to a higher temperature without producing ebullition, in one instance as high as  $175^{\circ}$ , with the formation of only one or two single bubbles. In this case a fragment of glass produced a copious ebullition, which continued until the fluid was cooled to  $125^{\circ}$ , when the effect was again reduced to the discharge of a few single bubbles; a cedar chip was then introduced, and produced a rapid ebullition. In one experiment, the three bodies, copper filings, fragments of glass, and chips of wood, were added in succession to the same portion of ether, and they each of them appeared to have the effect of producing ebullition when it had ceased from the action of the body previously employed. Plunging a thermometer into the ether caused the production of the bubbles at a temperature many degrees below the point at which ebullition took place without the thermometer, but the effect of the thermometer was, after a short time, no longer perceptible, and I observed that by alternately plunging the thermometer into the ether, and removing it from the fluid, the bubbles were produced at each immersion.

It appeared that in order to produce the full effect with the pieces of cedar wood, it was necessary that they should be perfectly dry; and I also found that wood which had been once employed, although perfectly dry, was not so powerful as fresh pieces: in one case by adding fresh bits of wood successively to a portion of ether, the boiling point was lowered from  $150^{\circ}$  to  $96^{\circ}$ .

The effect of the pieces of cedar appeared more remarkable by plunging into the same jar of water two tubes of ether, one without any addition, the other with the chips; in one experiment when the ether alone scarcely boiled at  $165^{\circ}$ , emitting only occasional single bubbles, the tube containing a piece of cedar was in violent ebullition; the temperature was gradually lowered; and even at  $102^{\circ}$ , the formation of a continued stream of small bubbles was very perceptible.

In order to observe whether any thing of a similar kind could be perceived with respect to alcohol, a portion of alcohol of specific gravity .848 was heated in a matrass over a spirit-lamp; a thermometer being immersed in the fluid stood at  $182^{\circ}$ ; the lamp was removed, and the ebullition ceased; but upon dropping into the alcohol a cedar chip, a fine stream of bubbles was observed to issue from it; the temperature of the fluid was now  $14^{\circ}$  below its former boiling point. Into another portion of alcohol which appeared to be near the boiling point, as was indicated by the appearance of rapid currents and by a slight hissing noise, a few copper filings were dropped; the ebullition was considerably promoted, and proceeded, as it were, by sudden starts, the bubbles always proceeding from the filings. In a later experiment, the boiling point of the alcohol was reduced as much as  $30^{\circ}$  and  $40^{\circ}$ , by dropping in successive pieces of the cedar wood.

I was desirous of ascertaining whether any analogous effect would be produced upon water, and for this purpose a small portion of well boiled distilled water was plunged into a flask of water that was in a state of rapid ebullition. The water in the tube was not perceptibly affected; the lamp which had been employed to boil the water being removed, the ebullition instantly ceased: some fragments of cedar were then dropped in, and perceptible streams of bubbles were, for some time, emitted from them. In a second experiment, copper filings were employed; a number of bubbles instantly attached themselves to the filings, and quickly rose to the surface, frequently carrying up the metal along with them. I will not, however, venture to determine how far, in the case of the water, the effect might depend upon a quantity of air still dissolved in it, or upon air which adhered to the surface of the bodies introduced, although from the quantity of effect, and the length of time during which it continued, I should scarcely think it ought to be altogether ascribed to this source.

I have simply related the facts as they occurred to me, without attempting any explanation of them; the results were new to me, and very unexpected; but I believe they will be found to be correctly stated.

I am, Gentlemen, yours, very truly,  
J. BOSTOCK.

*Feb. 21, 1825.*

Since the above was written, I have performed some additional experiments on the boiling point of water, which appear to shew in a more decisive manner than the former, that this fluid has the same property which I have noticed in ether, although in a much less degree.

A saturated solution of muriate of soda was heated over a lamp; at  $210^{\circ}$  it was in a state of strong agitation, and simmered loudly; at  $214^{\circ}$  single bubbles were discharged; at  $218^{\circ}$  or  $219^{\circ}$  it might be said to be in the boiling state, but the thermometer continued to rise until  $222^{\circ}$ , when the fluid was in strong ebullition. A test tube, containing water deprived of air by boiling, was plunged into the heated brine, and in a second or two it began to boil. The lamp was then withdrawn, when the brine soon ceased to boil, but the ebullition continued in the water for some time longer; it ceased at about  $218^{\circ}$  or  $217^{\circ}$ , but was instantly renewed by dropping in pieces of cedar wood. The brine was again placed on the lamp, and a test tube was plunged into it, containing a portion of water together with a thermometer. The water in the tube did not begin to boil until the thermometer had risen to between  $216^{\circ}$  and  $217^{\circ}$ , when ebullition first commenced; the fragments of wood were then

dropped in, and, as usual, very much increased the ebullition. The fluid was kept for some time at this temperature, and the extraneous bodies were alternately added to the water, and removed from it, when the ebullition was promoted or suspended accordingly for several times in succession. It would appear, therefore, that the boiling point of fluids, while under the same atmospherical pressure, is less uniform than has generally been supposed, and that it is materially influenced by the presence of extraneous bodies. In ether, this difference amounts occasionally to 50° or more, and in water to 4° or 5°. J. B.

## ARTICLE V.

*Astronomical Observations, 1825.*

By Col. Beaufoy, FRS.

*Bushey Heath, near Stanmore.*

Latitude 51° 37' 41.3" North. Longitude West in time 1<sup>h</sup> 20.93".

Jan. 22.	Immersion of Jupiter's first satellite . . . . .	{ 13 <sup>h</sup> 56' 21"	Mean Time at Bushey.
		{ 13 57 42	Mean Time at Greenwich.
Feb. 9.	Emersion of Jupiter's third satellite . . . . .	{ 11 59 56	Mean Time at Bushey.
		{ 12 01 17	Mean Time at Greenwich.
Occultation by the Moon.			
Jan. 29.	Immersion of a small star at . . .	4 <sup>h</sup> 02' 05.4	Siderial Time.

Observed Transits of the Moon and Moon-culminating Stars over the Middle Wire of the Transit Instrument in Siderial Time.

1825.	Stars.	Transits.	
Jan. 29.	—36 Tauri. . . . .	3 <sup>h</sup> 53'	56.9'
	29. —62 Tauri. . . . .	4 13	30.1
	29. —Moon. . . . .	4 24	52.58 First or West Limb.
	29. — $\pi$ Tauri. . . . .	4 31	47.83
	29. —243 Tauri. . . . .	4 47	14.65
	29. —295 Tauri. . . . .	4 57	29.38
Feb. 1.	— $\zeta$ Gemini. . . . .	6 03	46.38
	1. — $\delta$ Gemini . . . . .	7 11	39.6
	1. — $\gamma$ Gemini . . . . .	7 16	39.81
	1. —146 Gemini . . . . .	7 27	21.56
	1. —Moon. . . . .	7 32	30.35 First or West Limb.
	1. — $\delta$ Gemini. . . . .	7 36	01.61
	1. —224 Gemini. . . . .	7 41	48.51
	1. —1 Gemini. . . . .	7 45	29.41

## ARTICLE VI.

*Remarks on Solar Light and Heat.* By B. Powell, MA. FRS.*(Continued from vol. viii. p. 293.)*

(57.) In a former part of these remarks (16), I adverted to the experiments on the heating power accompanying or belonging to the different prismatic rays, this being one of the principal modifications to which the solar light has been subjected, and from which conclusions respecting the nature of its heating power have been deduced. On this part of the subject I propose now to make a few further observations with a view to ascertaining how far such conclusions may be substantiated, and will assist in forming a correct idea of the nature of the heating effects.

(58.) It is well known that the heating power belongs to the differently coloured rays in very different proportions. Among the results of different experimenters, there exists considerable discrepancy. The first person to whom we owe the idea of such investigations was the Abbé Rochon.—(See Phil. Mag. June, 1815, and Biot, *Traité de Physique*, tome iv. p. 600.) He found the maximum in the yellow orange rays. There is a much closer agreement however between subsequent observers, if we except the disagreement respecting the effect beyond the red rays.

(59.) The causes of these differences are to be in some measure sought in the different nature of the surfaces of the thermometers employed, or in the colour of the substances with which they were filled: as well as in the varying circumstances of the prism, &c.

Two coatings equally described as black may be very different in the shade of colour which they exhibit. If the tint incline to red for example, a less effect will be produced by the red rays.

(60.) The Abbé Rochon's result agrees with one which I have constantly obtained when the bulb was painted red. From the account of his experiments, Phil. Mag. June, 1815, it does not clearly appear what the nature of his thermometer was; but if, as I understand, it was filled with spirits, and they, as we may presume, were tinged red, his result is fully accounted for. My experiments were as follows:

*Indications of Differential Thermometer.*

Sept. 9.—9 a.m. Bulb coated with lake and vermillion.

Away. ....	5°
In the orange yellow .....	28
In the red. ....	25
Half inch beyond. ....	12

Two other experiments in which the same coating was thicker, gave

	Exp. 1.	Exp. 2.
Green . . . . .	12	—
Orange yellow. . . . .	18	12
Red . . . . .	16	11

(61.) That the heating effect produced within the limits of the visible spectrum is of the same kind as that produced by the solar light in its ordinary state; that is to say, that it is transmissible through glass, and affects a black surface more than an absorptive one, is, I conceive, sufficiently established by numerous experiments. I have frequently interposed a plate of glass, but without intercepting any perceptible portion of the effect on the photometer. A coating of brown or white silk also invariably gave a much less effect than Indian ink, or a surface of black glass.

These results seem to me decisive against the hypothesis of a superposition of two spectra, one of luminous, and the other of calorific rays.

(62.) It is obvious that the greater heating power displayed by the rays towards the red end of the spectrum, may be owing to either of the following causes, or to both jointly.

1. A greater intrinsic power of communicating heat.
2. A greater number of particles brought into action, or absorbed.

And this last cause may depend either upon the peculiar state of division to which the rays may be reduced, or upon a greater power of absorption in the surface for these than for other coloured rays, or here again both causes may co-operate.

With respect to the state of diffusion of the rays, it is obvious that the red rays are more concentrated than the yellow, and these more than the blue, &c.; so that from this cause alone we might expect a greater heating effect; a greater number of particles acting in the same space.

With respect to a possible increase of absorptive power in respect to the greater approach to the character of the extreme red light, I am not aware that we at present possess any results which can assist such an inquiry, unless we except the view maintained by Mr. Morgan in his experiments on the light from combustion, Phil. Trans. 1785, No. 11. He considers light as matter united to other bodies by attraction, blue rays having the least, and red the greatest affinity. If this view of the subject be admitted, *ceteris paribus*, more red particles would be absorbed than of any other coloured ray when impinging upon a surface.

(63.) A notice has very recently appeared (see *Annals of Philosophy*, Sept. 1824, p. 236) of some prismatic experiments by

Dr. Seebeck, of Berlin. These very important researches tend to establish the conclusion that the position of the maximum point of heat varies in the spectrum according to the nature of the dispersing medium. With some prisms it is situated in the yellow or orange, in those of crown glass in the centre of the red, and of flint glass beyond the red.

These experiments well explain the discrepancies between different observers, though other causes before adverted to may have had some share in producing those differences.

In viewing these results in reference to the *nature of the heating effects* accompanying the rays of light, it becomes desirable to inquire whether such changes in the heating power at different parts of the spectrum are accompanied by corresponding variations in the intensity of light: whether the greater heat be owing to a greater number of caloric and illuminating rays thrown into the same space, owing to the different law of dispersion followed by the different refracting media. It is very doubtful however whether there are any means of ascertaining this with certainty and accuracy by means of the illuminating powers, so as to arrive at any such conclusion. But if it were shown that the light is dispersed in different proportions to the same part of the spectrum by different prisms, and that such difference corresponded to the difference of heating power, Dr. Seebeck's results would in this case present no objection to the idea of the heating effect being inherent in the light, or resulting merely from light so modified as to become caloric.

The elaborate experiments of M. Fraunhofer on the refractive and dispersive powers of different substances (Edin. Phil. Journ. No. 18, Art. 16), exhibit instances of a considerable alteration in the relative dispersion of the rays by different media. This was ascertained with great precision by means of the well-defined bright and dark lines which he observed crossing the spectrum. It would be extremely desirable to ascertain what effect these lines have on the heating powers of the different rays.

If this view of the subject were not established, it might seem a natural inference that these results favour the idea of the heat being due to a separate set of rays; for if the heating power in the different parts of the spectrum can be made to vary, and the maximum can be thrown at pleasure into different coloured rays, it might be argued that the effect must depend upon some independent agent or set of rays distinct from the luminous rays.

Such a conclusion however is, perhaps, more than the facts will safely warrant. Those who have rejected the idea of separate rays of heat have usually gone to the opposite extreme, and supposed the heat to be identical with the light; and that the heating effect is merely the display of the same agent, light, in another form. But is this the necessary alternative? Is there

no medium between identifying light with heat, and maintaining a totally separate set of rays? It appears to me that if we reject either one of these opinions, we are not by any means obliged to adopt the other. Without identifying the two agents, or without supposing them inseparably united, without conceiving the heating power absolutely inherent in every particle of light, and invariable in intensity, except as the intensity of light varies, on the one hand; or on the other hand, that the heat consists of a distinct set of rays analogous to the rays of light; we may admit it to be in some very close state of union, combination, or dependence, yet so as to be susceptible of variation without a corresponding variation in the other effects of light. And such indeed, antecedently to the inquiry here adverted to, would seem the most natural and obvious way of considering the matter; because we are ignorant whether light be matter, or whether heat be motion, does it follow that there is any necessity for explaining the phenomena in which both agents seem concerned, by assuming them to be one and the same thing, on the one hand; or by denying that there is any sort of union between them, on the other?

(64.) To adopt a view of the subject which shall be a medium between the two extreme theories hitherto adopted appears to me not only to be what is most natural and most analogous to the views we take of other natural phenomena, but what is required by many strong facts.

To suppose that rays of heat exist distinct from those of light, either in the direct solar rays, or in the prismatic beam, requires the supposition of a new and peculiar sort of radiant heat, as different from common radiant heat as it is from light; by which means I do not see that we obtain any more satisfactory explanation of the phenomena than we did before.

(65.) It is certain that whatever we suppose to be the state in which the heat exists when it so inseparably accompanies the sun's light, there must be some peculiar circumstance in the mode of its union which makes its effects sensible only under some particular circumstances; and under others endows it with properties which heat in its simple radiant state does not possess.

In ordinary cases there is a direct communication of heat to substances with which light comes in contact. This effect is produced on all substances in some degree, but on some much more than others; and these are of a character widely different from those on which simple radiant heat is known to produce its greatest effects.

Heat accompanying light passes through the densest substances which are completely impervious to simple radiant heat (unless first thoroughly heated), and yet produces less heating effect on these than on any class of substances which are heated at all by the impact of light.

From these and many more examples which might be adduced, it is evident that heat accompanying solar light must be completely altered in its properties by the connexion subsisting between them.

(66.) If we had any experimental proof of the materiality of light, and should observe heating effects accompanying it, we should not hesitate to say that they were nothing more than an ordinary effect of a combination of heat with the material substance in question. But in the absence of such proof can we be permitted thus to describe the phenomenon? Did the question involve no other difficulty than this, I should reply that as we can define matter by no other tests than its observed properties, it would be the proper course for the experimentalist to deduce the nature of light from its observed properties, and not to describe those properties merely in conformity with its supposed nature. And observing real effects of ordinary heat, and finding them coextensive with the luminous beam, I do not see any real difficulty *on this ground* which should hinder us from describing the phenomenon as a combination of heat with the luminous particles.

It may be objected that to attribute such an union with heat to light is to assume the materiality of light, and thus to adopt gratuitous suppositions.

It is never objected, however, that we make hypothetical assumptions when we talk of ordinary matter possessing a sensible temperature or latent heat, &c. and yet what assumption do we make in the case of light which is not made here?

We conceive it allowable to say that ordinary matter is combined with heat, yet if we come to consider the matter accurately, it is only that we perceive a certain degree of solidity, extension, &c. united with a certain figure, and at the same time we find the sensation or effects of heat produced coextensively with those other properties cognizable by our other senses. Why then is it not allowable in the instance of light where we perceive a certain colour, extension, direction, &c. and heating effects concomitant and coextensive with the display of those properties, to say that light has heat in a similar sort of union with it?

(67.) In the preceding parts of these remarks, various proofs have appeared of the close connexion subsisting between the luminous rays and the heating effects accompanying them, and of the exact proportion followed so long as the light is of the same colour, and derived from the same source.

If then we can show by experiment that heating powers belong to light; if these effects accompany light in a manner and degree strictly analogous to a given class of those phenomena which arise from what we call an union of heat with ordinary matter; why should we not be permitted to describe the facts by expressions framed upon such analogy?

Those effects which we call effects of caloric in ordinary matter, pervade it in different ways, and are exhibited in several sorts of union or connexion. In order then to adopt with propriety this mode of describing the calorific phenomena of light, the chief point is to examine carefully whether the analogy does hold good; and to show to what part of the phenomena of heat in its combination with ordinary matter, those of its union with light are to be compared.

The first and most obvious idea is, can the effects be ascribed to what we might call the high *temperature* of light?

Since light is known to pass through many very dense media, and communicate very little if any heat to them, it might be inferred that it possesses no sensible temperature of its own; but this inference is obviously of no force: for in passing through transparent media, most of the luminous particles are never in contact with those of the medium, but pass probably between them and that with inconceivable velocity; so that whatever heat they may possess, they are incapable of communicating it. Some few rays are stopped and absorbed by the medium and more as it possesses a less perfect transparency; and in proportion as this is the case, we know that heat is always communicated, and all transparent bodies, after being some time exposed to the sun's rays, become heated.

When we come to consider the different development of its heating power on bodies of different colour, the effects are totally unlike those of temperature. On this principle, the heating effect would depend upon the impact of light rather than its absorption, and it should not be greater on a black than on a white surface. But perhaps the difference of calorific power in the prismatic rays is the strongest evidence against attributing the effect to temperature; for in this case how could such difference of temperature be maintained, supposing it could be originally communicated, when the rays are all in contact, and moving with equal velocities?

From these considerations, it would follow that the heat must exist in some state of combination with the light, more intimate and more connected with its changes and modifications than that belonging to heat of temperature.

In order to be the better prepared for following up this inquiry, I propose shortly to bring forward some experiments and conclusions, which are supplementary to some researches on light and heat from terrestrial sources lately read before the Royal Society. (See reports of the Royal Society, *Annals*, p. 224.)

(To be continued.)

## ARTICLE VII.

*On the Climate of the Antediluvian World, and its Independence of Solar Influence; and on the Formation of Granite.* By Sir Alexander Crichton, Knight, St. W. FRS. &c.

(Concluded from p. 108.)

HAVING endeavoured to prove, in the first part of this essay, that the laws of vitality, especially those to which the life of vegetables is subjected, afford an almost certain rule for judging of temperature; and having shown by the character of the fossil remains of the earliest plants of which we have any knowledge, that an uniformly high temperature exerted its influence over every part of the globe where they are found, I passed to the consideration of other geological facts, all of which are connected with the same subject, such as the similarity of the fossil remains in the transition and mountain limestone, and the different temperature of hot springs according to their respective depths, and the heat of waters which issue from rocks in deep mines. From all these facts, the conclusion appears to be inevitable, that in the very early periods of time, the heat of the earth was greater and more uniformly diffused, than can be accounted for by solar influence.

The analogy between crystalline substances (which we know to be of igneous origin) and granite, and the recent discoveries of Mr. Mitcherlich, were added as strong arguments in support of the doctrine. As chemical science has now opened a road by which we may account in a natural manner for the formation of granite, and also for the high temperature which resulted from its immediate production, we need not have recourse to any overstrained conjecture to account for the fact, such as the notion of a great and unaccountable change in the direction of the earth's axis, an idea which is totally unsupported by analogy or reason.

It is not possible for the imagination to conceive a state of chaos and disorder and of intense heat, like that which must have happened during the rapid ignition and oxidation of the metallic nucleus. Whether granite be the stratum of oxidized metals nearest the nucleus is very doubtful. From the examination of many collections of volcanic ejections, I am much inclined to think that some micaceous beds lie under granite.\*

\* The varieties of natural mineral compounds which assume the crystalline form of mica are numerous. If we except those compound substances which assume the form of garnets, there are none so diversified in their chemical constitution, and therefore there may exist micaceous forms under granite which differ from those that belong to it, or which lie over it, or are connected with other rocks. Masses of purely micaceous rocks appear to have been ejected from Vesuvius on its first bursting forth at the same time that pieces of granite were also thrown out.

If the supposition be well founded, that granite and its associates are of igneous origin, inasmuch as they are the result of quick oxidation and fusion, there ought not to be any great constancy in the super-position or juxta-position of these rocks, for it is clear that they may have varied according to the preponderance of any one metal, or any number of metals, in any given portion of the metallic nucleus.

Other causes appear to have co-operated with this in producing a considerable variety in the mechanical aggregation of the primitive rocks, as well as in their forms and relative position.

In a paper expressly written on antediluvian temperature, it cannot be expected that I should enter fully into an examination of all these causes; yet a cursory view of some of them is unavoidable for the elucidation of what is to follow.

The immediate effect of the oxidating process of the metallic mass would necessarily be a violent ebullition, agitation, and evaporation, of the surrounding fluid, and also the formation of various gases and gaseous oxides. Although the extinction of the ignition would result as soon as a crust of earthy oxides (the primitive rocks) was formed, yet during the consolidation of these, the action of the watery vapour, included between the intensely heated nucleus and the hot involucrum, would give an elastic force to the included vapour commensurate with its heat. When to this supposition is added the phenomena resulting from causes which we have every reason to believe to be similar, such as the sudden elevation of islands and of great tracts of land on the coasts, as well as the equally sudden depression of other tracts of continents, we are furnished with strong reasons for believing that many parts of the imperfectly solid and still heated granitic mass must have been elevated and rent in various places, giving birth to groups and chains of granitic mountains, the peaks of which, although greatly worn down since that period, still exhibit a character of ruggedness and rupture which peculiarly coincides with the theory.

The following account of the highest granitic peak in the Upper Oroonka district, taken from the justly celebrated Baron Humboldt's excellent work, entitled "*Personal Narrative*," is appropriate to the present subject, and so singularly interesting in itself, as to justify its insertion in this place. I may premise that the granitic peak called Duida is estimated by this scientific traveller at 1,300 toises above the level of the sea.

"The granitic summit of Duida is so nearly perpendicular that the Indians have vainly attempted the ascent. It is known that mountains the least elevated are sometimes the most inaccessible. At the beginning and at the end of the rainy season, small flames, which seem to change their place, are seen on the top of Duida. This phenomenon, which it is difficult to doubt on account of the agreement in the testimony concerning it, has

given this mountain the improper name of a volcano. As it stands nearly alone, it might be supposed that lightning from time to time sets fire to the brushwood; but this supposition loses its probability when we reflect on the extreme difficulty with which plants are set on fire in these damp climates. It must be observed, also, that these little flames are said to appear often where the rock seems scarcely covered with turf, and that the same igneous phenomena are displayed on days entirely exempt from storms on the summit of Guaraco, or Murcielago, a hill opposite the mouth of the Rio Tamatama, on the southern bank of the Oroonoko. This hill is scarcely elevated 100 toises above the neighbouring plains. If the assertions of the natives be true, it is probable that some subterraneous cause exists in Duina and Guaraco, that produces these flames; for they never appear in the lofty neighbouring mountains of Jao and Maraguanca, so often wrapped in electric storms.

“The first cause of these igneous phenomena is at immense depths below the secondary rocks in the primitive formations: the rains and the decomposition of water act only a secondary part. *The hottest springs of the globe issue immediately from granite.* Petroleum gushes from mica schist, and frightful detonations are heard at Encaramada, between the rivers Arauca and Cuchivero, in the midst of the granitic soil of the Oroonoko and the Sierra Parima. Here, as every where else on the globe, the focus of volcanos is in the most ancient strata; and it appears that an intimate connexion exists between the great phenomena that heave up and liquefy the crust of our planet and those igneous meteors which are seen from time to time on its surface, and which from their littleness we are tempted to attribute solely to the influence of the atmosphere.” —(See Personal Narrative, vol. v. p. 552 et seq. and vol. ii. chap. 5, p. 291, and vol. iv. chap. 14, p. 45.)

In the first part of this essay, it was stated in a general way, on the authority of Baron Humboldt, that the thermal springs of South America received their heat from the primitive rocks. The following passages are remarkable:—Speaking of thermal springs in the neighbourhood of the lake of Valencia, he says, “These springs gush out at three points of the *granitic* cordillera of the coast; near Onato, between Turnero and Maracay; near Murisa to the north-east of the Hacienda de Cura; and near Les Trencheras, on the road from Nueva Valencia to Porto Cabello. I could examine with care only the thermal waters of Mariara and Las Trencheras.” The mountains of Mariara, he says, “form a vast amphitheatre, composed of perpendicular rocks, crowned by peaks with rugged summits.” The granite which constitutes the peak of Calavera is separated, he assures us, by perpendicular fissures into prismatic masses.

These extracts I have inserted not with a view of proving any  
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analogy between the igneous phenomena of Duida and volcanos, but merely to justify the assertion concerning the deep fissures of granitic peaks, and the heat derived from their foundations: where it may be supposed there is a vicinity to the still hot nucleus of the earth.

The softening, elevation, and rupture, of the first formed granitic mountains, and the action of the agitated ocean, would produce the separation of an infinite number of minute grains of the newly-formed crystalline substances, many of which would be suspended mechanically for a longer or shorter time according to their respective gravity on the one hand, and the greater or lesser agitation of the waters on the other.

Some earthy oxides, such as the argillaceous oxide or clay, which have a kind of mechanical attraction for water, which is not perfectly understood, would be longer suspended than the minute crystals of mica, amphibole, quartz, or feldspar, and would be precipitated, all other things being alike, at a later period, and hence in the generality of cases gneiss lies under the argillaceous beds and rocks where these are found.

The presence of anthracite in the fissures of primitive rocks demonstrates that carbon was an elementary ingredient in the nucleus of the elementary globe; and it is therefore reasonable to conclude that, during the state of ignition, it would attract oxygen from the decomposition of the water, and form carbonic acid, which, after combining with the waters, would render it a solvent for all such metallic oxides as have a powerful attraction for it, and which are rendered more soluble through its agency, such as lime (oxide of calcium), and magnesia (oxide of magnesium).

The precipitation of such carbonated oxides (limestone and magnesian rocks) would depend chiefly on the agency of three well known causes; *first*, the continued formation of more oxides than the waters could dissolve; *secondly*, the diminution of temperature; and *thirdly*, the effects of evaporation.

These few principles throw much light on the formation of jaspers and serpentines of aqueous origin, and of the limestone rocks, especially if to such causes be added the heat of the subjacent rock on which they fell, and the pressure of the strata which were precipitated after them: and the same principles lead to an explanation of the various anomalies we meet with in the forms and relative positions of the primitive rocks.

All the formations from the granite to the deposits on which the diluvian boulder stones and gravel lie, demonstrate, by their organic remains, that there has been a gradual diminution of temperature from the earliest times till the earth was fitted for the creation of man, and the present race of animals, at which period it appears to have been entirely under solar influence and seasons.

During the long period of time comprehended between these remote points, the development of vegetable and animal life has passed through a great variety of remarkable forms, totally different from each other, and unlike those which exist in our days; but what peculiarly characterizes the living forms of the ancient world in contradistinction to the present races is, that in each epocha we meet with genera and species which have a perfect resemblance with each other over the whole surface of the globe, at least as far as it has been explored. The great distances of these parts which have been examined both as to latitude and longitude, justify in a great degree the accuracy of the assertion.

A minute examination of these ancient relics with those which bear the closest resemblance to them among our present races of vegetables and animals, seems to prove that the process has been from the simplest forms to the more complicated structures, and from those which require a constancy of heat and moisture to those which were fitted for great alternations of heat and cold, and for a great variety of soil.

As far as the great collection of facts which relate to the remains of organized bodies justifies their being generalised under this point of view, we seem to have a right to say, that the series of living forms which nature has observed is nearly as follow; first, a few plants of very doubtful character in the oldest greywacke slate; then zoophytes, and crustaceous moluscae with trilobites; afterwards an abundant creation of acotyledonous and monocotyledonous plants; after these a great increase of marine testaceous and crustaceous moluscae and zoophytes; then fishes, birds, and oviparous quadrupeds, comprehending the Saurian family; afterwards dicotyledonous plants; then marine mammalia; and lastly, terrestrial mammalia, and the present race of animals. The fossil remains of these lie buried in beds, which overlie each other, nearly in the order mentioned; and between the beds or strata are generally found others which do not contain any fossil remains, and which mark intervals of time in the process of their extinction.

The study of these remains and the strata in which they lie, cannot, I think, fail to produce an entire conviction on the mind of every impartial person, that their death was slow and gradual, there never having been at any one period a total and sudden destruction of the whole of the living races until the Deluge.

When the character of the vegetables and animals of the ancient world is duly considered in a physiological point of view as testimonies of temperature, we are led to the belief that the various living forms appeared in regular succession accordingly as the temperature of the earth suffered diminution; each succeeding race becoming fitted by its peculiarity of organization to support a colder climate, and increasing vicissitudes of heat and cold.

In the present state of the world, the ratio of dicotyledonous to acotyledonous and monocotyledonous plants is known to increase (all other circumstances affecting climate being alike) in proportion to the distance from tropical regions. In the cooler regions of the temperate zones, the proportion is as 60 to 1. In the torrid zones as 5 or 6 to 1. But in the very ancient world, all over the surface of the globe, we find nothing resembling a dicotyledonous plant until we come to the oolite, therefore, there is room to suppose that every part of the surface of the earth at that period was hotter than our hottest regions.

We now know from various facts that certain forms of vegetables and animals exist and multiply in a constant temperature which approaches nearly to the heat of boiling water. Dunbar and Hunter, in the journey they made along the river Ouachita, in Louisiana, found bivalves, and confervas, and other plants, in a hot spring, the temperature of which was between  $40^{\circ}$  and  $50^{\circ}$  of Reaumeur's thermometer. Sonnerat and Prevost state, that they discovered in the island of Lucon a stream of hot water of  $69^{\circ}$  Reaumeur, and that the roots of the agnus castus and a species of aspalatus grew in it. But a much more remarkable fact is mentioned by Forster, who found living plants growing at the base of a volcanic mountain in the island of Tanna, and that the heat of the soil in which they grew was  $210^{\circ}$  Fahr.

In the strata of the lias we meet with a rich collection of fossil remains, but among them there are none which prove the existence of any one terrestrial quadruped. There are plenty of crocodiles, and we are introduced for the first time since the formation of granite to the Saurian family.

Previously to entering into the consideration of these, it may be observed, that the laws of animal life do not afford the naturalist quite so certain a rule for judging of heat and climate as plants do; for every animal, from its being indued with a locomotive faculty, can roam to a great extent in quest of food, and is fitted to live where that can be found in sufficient abundance. Nevertheless we know of many, the health and existence of which force them to keep within certain boundaries of temperature. These, together with the antediluvian members of their families, are the only witnesses that can properly be brought forward to corroborate the testimony of the antediluvian flora.

The examination of the analogies which have a reference to this subject is attended with difficulty, and confessedly with some want of precision, merely from the vague and loose manner in which the denominations of the geographical zones are applied to the residence of animals. Some are described as inhabitants of the torrid zone, others of the temperate zone, others of the polar regions. In many cases, this is sufficient for general purposes; but as many genera and species of animals, both amphibious and terrestrial, are confined to a range of from  $12^{\circ}$  to  $20^{\circ}$ .

and as some live on the borders, of the temperate and torrid zones, but not in every part of each, these regions ought to be better described. \*

In the present essay, however, all that appears to be necessary is to point out the most striking instances of animals of hot climates which have an analogy with the fossil species of the same genera, and by stating the places in which their bones are found, to prove a similarity of temperature.

Before doing so, however, it may be right to call the attention of some readers to the consideration of an opinion which still prevails, notwithstanding all that has been written on the subject, and notwithstanding the late discoveries of the celebrated Prof. Buckland, which ought to have set the matter quite at rest for ever. The opinion is, that the remains of crocodiles, hippotamuses, opossums, rhinoceroses, hyenas, and other animals of hot climates, which are found all over Europe, were not the inhabitants of the regions in which their skeletons and bones are discovered, but that they were scattered over the surface of the earth after their death by some great destructive catastrophe resembling the Noachian deluge, of which several are supposed to have occurred.

Geology does not offer any collection of facts upon which it is possible to build an hypothesis of this kind; for although we find in the oldest conglomerate and greywacke fragments of primitive rocks (and this is the first or earliest appearance of any thing resembling diluvian detritus), yet the very agitated state of the waters occasioned by the intense heat of the subjacent strata would account for the phenomena. But allowing the argument its full force as to an analogy with a deluge, it is evident that it is not applicable to the question concerning the distribution of fossil remains, inasmuch as the creation of animated beings had not then begun.

The next great series of geological facts which bear testimony to the destructive agency of some powerful and general set of causes, is not met with until after the formation of the transition limestone. Soon after this period, a general convulsion of nature appears to have happened, leaving the most indisputable testimonies of its violence:—I allude to the complete rupture and dislocation of the newly-formed strata. Previously to their consolidation, these do not appear to have suffered any other disturbance during their formation than such as the gentlest motions of the waters would account for. The trilobites, and the few shells which are found in the transition limestone, are entire; and if the stems of encrinites and pentacrinites are broken and dispersed, it is a phenomenon which is capable of easy explanation, inasmuch as the weight and tenacity of the precipitated magma (carbonated oxide of calcium) would be sufficient to crush the slender stems of such zoophytes, and carry the fragments along with it to a short

distance, which corresponds with the relative situation in which the broken parts are found. That a stratum of a half liquid precipitate should be formed of nearly equal thickness in a highly inclined position is incredible; and we, therefore, have a right to infer, that this position in which it is commonly found was one into which it had been forced long after its perfect consolidation by the operation of some powerful causes.

There is one which may be reasonably conjectured to have exerted a great influence in producing the effect,—I mean the elastic vapours confined between the intensely heated metallic nucleus and the newly-formed crust of oxides. This may have acquired a force greater than the pressure which was acting on it, and to have burst its fetters, rupturing and overthrowing the superincumbent strata in the same manner as we find in our days whole tracts of land overthrown by subterraneous agencies of a probably similar kind. It is to this period that we must refer the elevation of continents and mountains, on the summits and surface of which we find proofs of their submarine origin; and it is to this period of general convulsion that we are also to look for the subsidences of other parts, forming the greater basins into which the ocean retreated, and the lesser basins which afterwards were filled with fresh water torrents and rain. But at this period of time the great work of creation had made but little progress, and the only animals which existed belonged to the sea. None appear to have been destroyed by this great catastrophe, and if we find a difference between the zoophites and marine molluscae which were deposited afterwards, there is no way of accounting for the phenomenon but in the diminution of temperature which was gradually taking place.

Since this period of disorder until the appearance of the diluvian boulder stones and gravel, I do not know of any geological appearances which have the most distant resemblance to the wrecks of a deluge. The work of creation, on the contrary, appears to have proceeded with great regularity, varying and multiplying the living forms according as the temperature varied, and as dry land and alluvial soils were produced.

It is impossible to deny that many ancient continents and alluvial deposits have been frequently overflowed both by salt and fresh water. They have left indisputable testimonies of the fact. But these were all of them partial in comparison with the two events described, or with the deluge; and that the animals, the remains of which they covered with new deposits, were dead before the inundations, appears from the perfect state of their skeletons.

When to these considerations are added the late remarks of Prof. Buckland on his discovery of the dens of antediluvian hyænas, &c. in this country, no doubt can be left on the mind of an unprejudiced person, that the animals of hot climates, the fossil bones of which are found distributed over both continents,

and in every degree of latitude, were, in ancient times, the natural inhabitants of the places in which their remains are discovered.

Alligators and crocodiles, it is well known, are confined by their nature to the very hottest regions of the earth. They are chiefly found in the Niger, the Nile, the Ganges, the Amazone, and other rivers of the torrid zone. So dependent are they on a hot temperature, that it has been found impossible to protract their lives beyond a very short period when brought into a temperate one, except by artificial temperature. Bonnard, in his *Dictionnaire d'Histoire Naturelle*, copies the following passage from M. Perrault's account of a living crocodile which was brought to Versailles. It is so much to the point that I cannot avoid inserting it:—"Disons d'abord, que le spectacle de cet animal vivant, déjà si propre par lui-même à exciter la curiosité, parut surtout extraordinaire par la circonstance de la saison où l'on étoit alors, et par celle du climat. Car le froid est tellement contraire au crocodile qu'en Amérique et en Égypte même, au rapport des auteurs, cet animal ne peut passer les nuits d'été que dans l'eau, qui alors est beaucoup plus chaude que l'air. Ceux qui avoient apporté par terre depuis le Rochelle, le crocodile dont il s'agit, dirent qu'ils l'avoient cru mort plusieurs fois, et n'avoient pu le faire revenir qu'en le mettant auprès du feu." This crocodile lived only a little more than a month.

The living crocodile is never found in any part of Europe, but its fossil remains are discovered all over it, and in various beds.

The fossil remains of a species of didelphis or opossum have been found in the oolitic beds of England. No living opossum is ever found in a corresponding latitude, nor indeed do any exist in Europe. The living species are chiefly inhabitants of South America, and are principally found in Brasil, Guiana, Mexico, and range into Virginia.

The chief residence of the hippopotamus is in Africa, between the river Senegal and the Cape of Good Hope, and in several tropical rivers of Asia. The bones of the antediluvian hippopotami are found in the upper valley of the Arno in great abundance; and as Baron Cuvier assures, in almost as great numbers as those of rhinoceroses and elephants. They are also frequently met with in the neighbourhood of Rome, and in the county of Middlesex, in the neighbourhood of Brentford.—(See Mr. Trimmer's account of them in the *Phil. Trans.* for 1813.) Along with these there were also found the bones of rhinoceroses and elephants. As to fossil elephants' bones, they are found all over the continents of Europe and America. Not only European Russia, but almost all Siberia, teems with them.

It is surely needless to multiply facts of this kind. If more be required, the reader is referred to the classical and truly

philosophical works of Baron Cuvier, especially to his *Recherches sur les Ossements Fossiles*.

The fossil remains found in one of the uppermost of our strata, the London clay, indicate for all the places in England, as also for others on the continent of Europe where contemporaneous deposits are met with, a temperature equal to that of the West Indies and the north of Africa. In these deposits the fossil remains begin to bear a close analogy to living genera and species.

We have no means of measuring the lapse of years from the period of these depositions to the creation of man. From the time of the Deluge to the birth of Christ is 2348, according to the Hebrew text, and consequently 4173 years from the present date. The creation of man is supposed to have been 1656 years before the Deluge, making altogether 5829 years since Adam. Now supposing a period of 1000 years to have elapsed from the extinction of those races of animals to the creation of man, we have a period of 6829 years, during which the climate of Great Britain has been reduced from the heat of the West Indies, or the north of Africa to its present standard.

The whole surface of the earth seems to have suffered a great diminution of temperature by the action of the Deluge, the waters acting as a medium between the earth and its surrounding atmosphere. On the retreat of the waters, another cause of cold arose in the immense evaporation which followed; and as the radiation of heat from the centre of the earth was constantly going on, we have a right to presume that the equality of temperature on the surface of the earth was greatly destroyed by that catastrophe, and that the loss of terrestrial heat has been much more rapid since the Deluge than in an equal lapse of time preceding it. Solar heat is insufficient to compensate the loss of caloric in the polar regions where the fields of ice seem constantly increasing.

But at the period of the deposition of the London clay, and its contemporaneous formations, it appears probable from the animal remains they contain, that the heat on the surface of the earth was not much greater at the time of their existence than it is at present in places which are inhabited by many of the human race. If the earth was not then fitted for man, it must have been owing to other causes than mere temperature; it could not have lost much heat by radiation between that period and his creation.

According to the Hebrew text, the human race began to be renewed after the Deluge in those regions where solar influence is great, and consequently in a temperature which corresponded the most with that which had been nearly universal over the earth at his creation and till the Deluge.

At present the loss of terrestrial heat is so great that we are

wholly dependent on solar influence. The glaciers descend the mountains; and regions which were green with vegetation, and inhabited, are now wholly frozen and deserted. The reflections to which this leads would be entirely out of place. My object has been merely to throw together a collection of remarkable facts in geology, which it appeared to me to be time to generalize, so as to become more intelligible, and as elucidating each other. How far I have succeeded must be left to the decisions of those competent judges who peruse your journal, and whose remarks and criticisms I shall receive with pleasure.

### ARTICLE VIII.

*Examination of a Mineral from Sussex County, New-Jersey.*

By Prof. Renwick.\*

THE substance in question exists intimately connected with, and disseminated through the ore of the Andover mine; an ore that was at one period famous for producing the best iron in North America, and the only kind from which steel has been successfully manufactured.

This ore appears, at the first glance, to be composed of three very distinct substances. The first is intermediate in appearance between granular Franklinite and large-grained magnetic iron ore: on a cursory examination, it seems to be a protoxide of iron with a slight trace of zinc. The second is an amorphous quartz, tinged with a colour varying from a pale rose colour to a deep vermilion. The third is of a dull vermilion red, and of a granular fracture; in some specimens fine, in others coarse-grained. This last was chosen as the subject of examination; it is hard enough to scratch glass; its powder is rose red; it slightly affects the magnet; and it effervesces with acids. It had been supposed to be a red oxide of zinc. My first experiments showed that it had no analogy with that substance, and it having been subjected to the action of the blowpipe by Dr. Torrey, he inferred that it contains cerium, as it formed with borax a glass that was green while hot, but lost its colour on cooling. Exposed alone to the blowpipe, it is infusible.

To ascertain its nature, it was subjected to the following preliminary process:

#### A.

(1.) A small portion was separated, and reduced to fine powder in a steel dish. In this state it was acted upon with violent effervescence by nitro-muriatic and muriatic acids; giving with the latter the peculiar smell of hydrogen. The action ceased in

\* Annals of the Lyceum of Natural History, New York.

about half an hour, leaving a considerable part of the mass undissolved, and but little altered in appearance.

The muriatic solution being acted upon: by tests showed, among others, the following phenomena :

(2.) With ferrocyanate of potash a copious blue precipitate.

(3.) With ammonia a precipitate of a rich vermilion red.

(4.) With carbonate of ammonia a reddish white precipitate.

(5.) With hydrosulphuret of potash, a milky appearance, that, subsiding, left a scanty brown precipitate.

(6.) The compounds of cerium being soluble in excess of acid, the nitro-muriatic solution was concentrated until the greater part of the free acid had evaporated, and was then neutralized to the point of nascent precipitation by carbonate of soda.

(7.) A part of the liquor in No. 6 being diluted, crystals of sulphate of soda were thrown in ; these, after some hours, were dissolved, causing a white precipitate.

(8.) To another portion of the concentrated and neutralized nitro-muriatic solution (6) tartrate of potash was added, on which a copious white precipitate ensued. The suspicion that the substance contained cerium being thus confirmed, it was subjected to a more strict examination, as follows :

## B.

(1.) A mass weighing nearly an ounce, and containing a very few small grains of the oxide of iron, was broken from the corner of one of the specimens. Weighed by means of a very accurate hydrostatic balance, it appeared to have a specific gravity of 3.25.

(2.) This mass being first crushed into fragments in a steel mortar, all the extraneous matter was carefully picked out with a forceps ; it was then reduced to impalpable powder by long grinding in an agate dish.

(3.) Fifty grains of the powder were boiled for half an hour in nitro-muriatic acid, the solution assumed a rich yellow colour, and a considerable residuum was left, which, separated, washed, and dried, had lost in weight exactly 27 grains.

(4.) The insoluble portion (3) was then put into a silver crucible with 70 grains caustic potash ; water being thrown on, the mixture was boiled, evaporated to dryness, and finally fused. The fused mass was softened by water, and separated from the crucible ; muriatic acid being then added, the solid matter swelled up into a gelatinous mass. This was evaporated to dryness, being constantly stirred throughout the process, and afterwards boiled for two hours in very dilute muriatic acid. The whole was then thrown upon a filter, and carefully washed ; the insoluble portion when dry was found to weigh 16.3 grains, was white, with a faint and hardly perceptible tinge of rose colour.

(5.) The nitro-muriatic solution and washings (3), and the

muriatic solution and washings (4), having been mixed, liquid ammonia was added in excess, which threw down a red precipitate; the supernatant clear liquor was poured off; the residue thrown on a filter and washed, and the liquor with the washings set by for further experiment. See (12).

(6.) The precipitate (5) was redissolved in a small quantity of muriatic acid, and the solution concentrated; tartrate\* of potash was added until effervescence ceased, when crystals of tartaric acid were thrown in, by which a copious white precipitate was produced.

(7.) This precipitate (6) was decomposed by heat, which, being pushed too hastily, a portion of charcoal was left; lest any carbonate of potash should be present, in consequence of a portion of the precipitate (6) being the difficultly soluble bitartrate of potash, the mass was washed with very weak vinegar. To separate the carbon, the mass was again acted upon by a small quantity of muriatic acid, and the solution filtered.

(8.) The new muriatic solution was decomposed by ammonia, which threw down a red precipitate, that, when washed and dried, weighed 6.16 grains.

(9.) Lest the acetic acid (7) had carried off any part of the mineral, it was tested with ammonia, but no precipitate ensued.

(10.) Into the liquor remaining after precipitation by tartrate of potash and tartaric acid (6), ferrocyanate of potash was dropped; a milky appearance first took place, and finally a copious precipitate of a pale blue colour. This precipitate, when dried, weighed 28.9 grains; which, supposing it to be a ferrocyanate of the protoxide of iron, and its equivalent number 99, gives on reduction 10.5 grains protoxide of iron.

(11.) To the liquor yet remaining (10), carbonate of ammonia was added; a white powder was thrown down, weighing 1.84 grains.

(12.) The ammoniacal liquor and washings after the first precipitation (5) were boiled for an hour, but no precipitate ensued; being then acted upon by carbonate of soda, a greyish precipitate fell, weighing when dried 20.92, and manifesting the presence of 12.04 grains caustic lime.

### C.

(1.) Another portion of the powdered mineral was exposed for an hour to a red heat in a platina crucible; its weight was reduced from 50 to 48.25 grains.

(2.) It was then treated as before (B. 3) with nitro-muriatic acid, the insoluble portion fused with caustic potash, &c. as in (B. 4), and the whole of the liquors united, as in (B. 5). The mixture was then concentrated to separate the excess of acid, and again diluted with water. Crystals of sulphate of soda

\* Query Carbonate?—C.

thrown in were, after a few hours, dissolved; a precipitate ensued, which, washed, and dried, weighed 8.62 grains. If the representative number of cerium be 92, this, when reduced, will give 6.06 grains of peroxide of cerium.

(3.) The liquor whence the cerium had been precipitated (2), being tested by the oxalate and benzoate of ammonia, showed the presence of lime and protoxide of iron.

The more important results of the analysis B. being thus confirmed, it was not considered necessary to extend the process farther. The results may be, it is believed, depended upon, except so far as the equivalent numbers of ferrocyanic acid and cerium enter into the calculations of (B. 10) and (C. 2), and the doubt whether a small proportion of some other metal may not have been thrown down in (B. 10). This analysis shows the following to be the constitution of the substance :

	Grains.
Silex, B. (4) . . . . .	16.30
Peroxide of cerium, B. (8) . . . . .	6.16
Protoxide of iron, B. (10) . . . . .	10.50
Alumine, B. (11) . . . . .	1.84
Lime, B. (12) . . . . .	12.04
Water, C. (1) . . . . .	1.75
Loss . . . . .	1.41
	50.00

As this mineral neither agrees in external characters nor chemical constitution with any other compound of cerium, that has been hitherto described, I have little hesitation in announcing it as a new ore of that metal. It appears to possess the nearest analogy in its composition with the *Allanite*. This last, by the analysis of Thompson, quoted in Macneven's edition of *Brande's Chemistry*, has in 50 parts,

Silex . . . . .	15.80
Oxide of cerium . . . . .	15.13
Alumine. . . . .	1.83
Protoxide of iron . . . . .	11.34
Lime . . . . .	4.11
Water. . . . .	1.79
	50.00

If my surmise in this respect be true, I should propose to name it the *Torrelite*, in honour of my friend Dr. John Torrey, to whom mineralogical science is under many important obligations, and to whom this tribute is fairly due, as it is to his nice tact in the management of the blowpipe, that the discovery of cerium in the substance is to be attributed.

*Observations on the Analysis of Torrelite.* By J. G. Children, FRS. &c.

Several months before the second number of the *Annals of the Lyceum of Natural History of New York* (from which we have copied the preceding article), arrived in this country, my friend Mr. J. F. Daniell, received a specimen of Torrelite from Professor Renwick, which he put into my hands for examination with respect to its containing oxide of cerium. I shall briefly state the results of the experiments to which I submitted it.

Heated to redness in a small matrass, the assay gave off a little water; it did not decrepitate, nor suffer any change in its appearance.

Before the blowpipe, *with soda*, on the platina wire, it gave in the oxidating flame an opaque deep green globule, rather inclining to blue; by the addition of nitre the colour became pure deep green. In the reducing flame, the assay became brown.

*With borax*, it dissolved readily; in the oxidating flame the globule was transparent, and of a fine amethyst colour. In the reducing flame, light yellow whilst hot, and colourless when cold. It remained perfectly transparent.

*Salt of phosphorus* had very little action on a small fragment of the assay; the globule in the oxidating flame was quite transparent; yellow hot, colourless cold. In the reducing flame, colourless both hot and cold. The fragment remained enveloped in the diaphanous glass, apparently very little altered. A portion of the assay reduced to fine powder was more readily acted on by the salt of phosphorus than the fragment, but the appearances were similar, except that the colour was rather deeper. A considerable silica skeleton remained in the fused globule, which, when cold, was slightly opaline.

*Alone in the forceps*, the mineral fused with difficulty on the surface, bubbled up, and became covered with a vitrified greenish grey transparent coating.

These experiments give no indication of the presence of oxide of cerium, but as that substance, when in combination with iron and silica, cannot be detected by the blowpipe, no certain inference, as to its presence or absence, could be drawn from them. They demonstrate, however, that the mineral contains manganese in considerable quantity, of which the analysis takes no notice.

It is stated in Mr. Renwick's paper, that Dr. Torrey inferred, that the mineral "might contain cerium as it formed with borax a glass that was green whilst hot, but lost its colour on cooling." The characters which Berzelius gives of oxide of cerium with borax before the blowpipe are, "that it forms in the exterior flame a beautiful red or deep orange yellow glass, whose colour fades

on cooling, and is ultimately reduced to a yellowish tint; by flaming, the glass becomes enamel white. In the reducing flame it loses its colour.\* That these characters are accurately given, I can vouch from experiment.

A portion of the mineral in fine powder was digested in nitro-muriatic acid to dryness. The dry mass was redissolved in water, with the addition of a little muriatic acid, and a few drops of nitric. A part remained undissolved, which on examination proved to consist of silica with a little oxide of iron and oxide of manganese. This being separated by the filter, ammonia was added to the clear solution, which threw down an abundant dark red precipitate. (a). The whole was thrown on the filter, and the ammoniacal solution set aside. On standing a few hours, it deposited a portion of pure oxide of manganese; and on the addition of oxalate of ammonia, afforded an abundant white precipitate, which was found to consist merely of oxalate of lime with a considerable quantity of oxalate of manganese.

The dark red precipitate (a) was redissolved in muriatic acid with a few drops of nitric acid, the solution carefully neutralized by ammonia, and an excess of oxalate of ammonia dropped into it. At first that reagent occasioned no precipitate, but after a large quantity of the oxalate had been added, the solution became turbid, and on standing deposited a small white precipitate. This was separated by decanting off the supernatant fluid and well washed. On examination it proved to be nearly pure oxalate of manganese, for being heated in a platina capsule over the spirit lamp to redness, it left a dark brown substance, which gave with soda and nitre, before the blowpipe, an opaque dark green globule in the oxidating flame, and with borax a transparent one of a beautiful amethyst colour, which disappeared when heated in the reducing flame.

Ammonia added in excess to the solution from which the last precipitate had been separated, threw down a large quantity of red oxide of iron mixed with a little manganese, and on pouring in a solution of prussiate of potash to the ammoniacal liquor, previously filtered, a considerable white precipitate of prussiate of manganese was immediately formed.

As my object was merely to ascertain whether oxide of cerium be present in the mineral or not, the quantities of the several precipitates were not attended to; the *quality* of each, however, was carefully examined, and no trace of cerium could be detected in any of them.

A portion of *cerite*, similarly treated, instantly gave an abundant precipitate of oxalate of cerium, on adding oxalate of ammonia to the nitro-muriatic solution, previously neutralized by ammonia.

\* The Use of the Blowpipe in Chemical Analysis, &c. p. 100.

The results of these experiments were communicated by Mr. Daniell to Prof. Renwick, who had the goodness to send me another specimen of Torrelite, and I received at the same time a copy of his analysis, published in the work already alluded to.

The results which I had obtained differing so much from those of Prof. Renwick, I considered it due to that gentleman to resume my labours, and I accordingly repeated his analysis on a portion of the mineral he had so liberally and obligingly furnished me with.

To my surprise, I was as unsuccessful as before in my attempts to discover any oxide of cerium. I therefore requested Mr. Faraday to have the goodness to examine a portion of the mineral, who informs me that he also has been unable to detect in it any trace of the oxide in question. To whatever cause it may be owing, therefore, I am compelled to conclude that some error has crept into Prof. Renwick's analysis, and that oxide of cerium forms no part of the constituent ingredients of Torrelite.

It may be right to add, that both mine and Mr. Faraday's experiments were made on the dull vermilion red portion of the mineral.

J. G. C.

## ARTICLE IX.

### *Proceedings of Philosophical Societies.*

#### ROYAL SOCIETY.

*Jan. 27.*—The name of the Solicitor-General was ordered to be inserted in the printed lists of the Society; and a paper was commenced, On the Anatomy of the Mole-Cricket; by John Kidd, MD. FRS.

*Feb. 3.*—The reading of Dr. Kidd's paper was concluded; and an Appendix to the Croonian Lecture, by Sir E. Home, Bart. VPRS. read, announcing the simultaneous discovery by himself and Mr. Bauer, of nerves in the human navel-string and placenta, drawings of which by Mr. B. were annexed to the paper.

*Feb. 10.*—Lord Viscount Strangford, and the Rev. George Fisher, MA. were admitted Fellows of the Society; and a paper was read, of which the following is a brief abstract:—

Notice of the Iguanodon, a Fossil Herbivorous Reptile found in the Sandstone of Tilgate Forest: by Gideon Mantell, FLS.: communicated by Davies Gilbert, Esq. VPRS.

In the sandstone of Tilgate Forest, near Cuckfield, in Sussex, which belongs to the iron-sand formation, and forms part of a chain of hills extending from Hastings to Horsham, are found the teeth and a few of the bones of the subject of this paper, together with those of a gigantic species of crocodile, of the megalosaurus and the plesiosaurus, and the remains of turtles,

birds, and vegetables. The author, some time since, sent specimens of the teeth to various naturalists; in particular to M. le Baron Cuvier, whose opinion of them coincided with his own, that they belonged to an extinct herbivorous reptile hitherto undescribed. With the assistance of Mr. Clift he had subsequently compared them with those of a skeleton of the recent Iguana of the West Indies, in the Museum of the Royal College of Surgeons, with which he found them to possess a close affinity; and he details, in this notice, the particular results of the comparison; adverting, also, to the probable station of the extinct animal in the order of Saurians. From the affinity just mentioned, and at the suggestion of the Rev. W. D. Conybeare, he had given it the name of *Iguanodon*. On the supposition that the proportions of the parts in the extinct animal were the same as in the recent, Mr. Mantell infers that the *Iguanodon* must have exceeded in size even the megalosaurus, and have been upwards of sixty feet in length. From the fossils associated with its remains, he concludes, that if an amphibious, it was not a marine reptile, but inhabited rivers and fresh-water lakes. Drawings of the teeth and bones of the *Iguanodon* were annexed to this communication.

*Feb. 17.*—Capt. J. Mangles, R.N. was admitted a Fellow of the Society; and a paper was read, entitled “An Experimental Inquiry into the Nature of the Radiant Heating Effects from Terrestrial Sources;” by the Rev. Baden Powell, MA. FRS.

The object of this paper is to investigate an important question relative to the nature of the heating effect, radiated or emitted from burning and incandescent bodies.

The heat from non-luminous sources has been shown by Professor Leslie to be entirely intercepted by a glass screen; that from luminous hot bodies, though in a considerable degree intercepted, is yet partially transmitted. M. de la Roche has shown, that the part transmitted increases in proportion to the part intercepted, as the body under trial approaches nearer its point of luminosity, or is more perfectly luminous; and both M. de la Roche, and his commentators, seem disposed to view these results as showing that the effect is due to one simple agent, the principle both of light and heat gradually passing from the state of the latter to that of the former, and in proportion becoming capable of passing through glass. This opinion, however, is not absolutely proved; and the facts *may* be explained without adopting it. Luminous hot bodies *may* give off two separate sets of rays, or emanations, one of light possessing an inseparable heating power like the light of the sun, and transmissible through glass; the other, simple radiant heat totally stopped by glass. To examine which of these hypotheses is the true one, was the primary object of the experiments here

undertaken. The principal experiments were conducted in this manner; two thermometers, coated one with smooth black, and the other with absorptive white, were exposed together under exactly similar circumstances to the radiation from different luminous hot bodies, such as iron raised to a considerable degree of incandescence, and the flame of a lamp. This was done first with, and then without the interposition of a glass screen. After allowing for all the causes of error likely to have affected the results, the object was to observe the *ratio* of the rates at which the two thermometers rose when exposed; and to compare it with that similarly obtained when they were screened. If the screen (according to the theory of de la Roche, &c.) only intercepted a portion of one simple agent, the screened effects would be merely diminished in *absolute quantity*, but would remain unaltered in *ratio*. If this be not the case, it will follow, that the transmitted portion of heating influence not only differs from the rest in the property of transmissibility, but also affects surfaces by a different law.

In all the various experiments tried, one uniform result was obtained; viz., that the screened ratio was much greater than the exposed. For example, in one instance, with the flame of a lamp the screened ratio was about  $\frac{\text{white}}{\text{black}} = \frac{1}{4}$ ; the exposed  $\frac{\text{white}}{\text{black}} = \frac{1}{1.8}$ .

The general conclusion deduced is, that the radiant heating influence is the united effect of two distinct agents; one is simple radiant heat, having the properties of relation to *texture* and not darkness of colour, and *stopped by glass*; the other, having relation to *colour*, and *transmitted by glass*, which may be denominated "transmissible heat," or (from its close association with light,) "the heating power of light."

The distinct existence of two heating causes in the total effect from luminous hot bodies having been established in the first part of this paper, the object of the second part is to ascertain and compare the *ratio* subsisting between those two parts of the heating effect in different instances. The instances tried are, the flame of a lamp, when the combustion was in different degrees of completeness; the union of several flames compared with one; the increase of density in a flame; and the different degrees of incandescence in metals.

In all these instances, a regular increase of *ratio* was observed, in correspondence with an increase in the completeness of combustion, with the junction of different flames; with the increase of density in a flame; and with the degree of incandescence in metal.

Similar conclusions were also inferred from the experiments of Mr. Brande, Count Rumford, &c.

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In the conclusion deduced from the whole, that part of the heat which belongs to the light is shown to be derived from the hot body, and to be abstracted, or made to disappear from its sensible temperature, so as not to be given off as radiant heat. We have no right to assume that this portion of the heat is converted into light. But it is evident that it exists in some state of very close combination with light; it is never rendered sensible till the light is absorbed, as by dark coloured bodies. All bodies become luminous by the application of a certain degree of heat. We can then form no other conclusion, than that the portion of the heat which is in the first instance lost, is, in fact, communicated in some way to the light, and this can be in no other way than by becoming latent in it; and in fact thus giving it the form of light; from which it is again given out and rendered sensible when the light is absorbed, or changes its state and enters into combination with other bodies.

This view of the subject is applicable to a variety of phenomena. Those of phosphorescence (hitherto considered so anomalous) are noticed. Most others are too obvious to require particularizing.

These conclusions will perhaps be regarded as arguments in favour of the materiality of light, it being thus shown to possess those properties in respect to latent heat, which would belong to a substance of immense tenacity and elasticity.

Feb. 24.—The reading was commenced of a paper, On the Materno-fœtal Circulation; by David Williams, MD: communicated by Dr. John Thomson, of Edinburgh, FRS.

#### LINNEAN SOCIETY.

The sittings of the Linnean Society for the Session 1824-5, were resumed on Nov. 2, when the following papers were read:

A letter from Mr. J. De Carle Sowerby, FLS. to Mr. R. Taylor, Sec. LS. stating that many specimens of a fresh-water shell, the *mytilus polymorphus* of Gmelin, which is a native of the Danube, had been found attached to timber in the Commercial Docks, where the species had probably been brought in timber.

A Description of three Species of British Birds, two of them new to the Ornithology of the British Isles; by N. A. Vigors, Jun. Esq. A.M. FLS.: communicated by the Zoological Club. The birds described in this paper are, *Anthus Richardi*, Vieillot, two specimens of which were taken a few years ago at Kingsland, near London; an undescribed *Scolopax* shot in Queen's County, in Ireland, in 1822, and named by Mr. Vigors, *S. Sabini*; and *Querquedula glaucitans*, or the Bimaculated Duck, taken in a decoy near Maldon within these few years.

A Description of *Couania*, a new genus of plants; and of a new species of *Sieversia*: by Mr. D. Don, Librarian to the Linnean Society. These remarkable plants, belonging to the

family of Rosaceæ, are contained in an extensive collection from Mexico, part of the Herbarium of Mocinno and Sessé, now in the possession of A. B. Lambert, Esq. VPLS.—*Cowania*. Char. essent. Calyx 5-fidus. Petala 5. Ovaria 5-7, ovulo erecto. Styli terminales, continui. Achenia stylis plumosis persistentibus aristata. Embryo erectus.—*Siegersia paradoxa*, foliis fasciculatis linearibus obtusis sessilibus integris 3-5-fidisve, floribus subcorymbosis, stylis plumosis, caule fruticoso.

Nov. 16. —A letter was read from John Atkinson, Esq. FLS. to Alexander Macleay, Esq. Sec. LS. accompanying specimens of a beetle found in a mummy sent from Egypt by Mr. Salt, and recently opened for the Museum of the Leeds Philosophical Society. The imperfection of the embalming appeared to indicate that the person had not been of high caste: the folds of the linen in which it was wrapped contained several hundreds of the larvæ of the beetle, and some of the perfect insects.

Descriptions of several species hitherto unpublished of the genus *Coccinella*; by George Milne, Esq. FLS.: communicated by the Zoological Club. The new species described in this paper, were *C. circumdata*, *4-fasciata*, *ephippia*, *parva*, *6-guttata*, *decussata*, *abdominalis*, and *cyanca*, from Brazil; *28-maculata* and *18-maculata* from New Holland; *cordata* and *comata* from North America.

An Account of some plants belonging to the Natural Order called by Dr. Jack Cyrtandraceæ; by Francis Hamilton, MD. FLS. The species described in this paper are *Chelone filiforme*, *C. rubicunda*, and *C. latifolia*.

Observations on the *Motacilla Hippobis* of Linneus; by the Rev. Revett Sheppard, MA. FLS.: communicated by the Zoological Club. Mr. Sheppard concludes, from a particular examination of the synonymy, &c. of this species of *Motacilla*, that it is the Greater Pettevchaps of English writers.

Dec. 7. —Mr. G. B. Sowerby, FLS. exhibited some Beryls, from the Morne Mountains, in the county of Down, in Ireland.

The reading of Dr. Hamilton's Commentary on the third part of the Hortus Malabaricus was continued; the following were among the plants, the history and synonymy of which were investigated: *Coddia Panna*, *Niti Panna*, *Toddia Panna*, *Katou Indel*, *Tsjaka Maram*, *Ata Maram*, *Anona Maram*, *Ansjeli*, *Kato Tsjaka*, &c.

Dec. 21. —A letter from J. Youell, Esq. ALS. stating that specimens of *Ardea Cayanaensis* and *Tantalus viridis* had been taken near Yarmouth, and deposited in the Norwich Museum; and correcting some erroneous statements of Bewick respecting *Fulica atra*.

An Account of a remarkable Fungus; by the Rev. W. Kirby, MA. FR. and LS. Mr. Kirby gives the name of *Atractus* to this fungus, and places it between *Clathrus* and *Phallus*.

A Description of such Genera and Species of Insects, alluded to in the Introduction to Entomology of Messrs. Kirby and Spence, as appear not to have been before sufficiently noticed or described; by the same author: communicated by the Zoological Club.

Jan. 18, 1825.—The reading of the Rev. Messrs. R. Sheppard's and W. Whitear's Catalogue of the Birds of Norfolk and Suffolk, commenced and continued during the last Session, was resumed.

Feb. 1.—On the Structure of the Tarsus in the Tetramerous and Trimerous Coleoptera of the French Entomologists; by W. S. Mac Leay, Esq. M.A. FLS.

Feb. 15.—The reading of the Rev. Messrs. Sheppard's and Whitear's Catalogue of Norfolk and Suffolk Birds, and that of Dr. Hamilton's Commentary on the third part of the Hortus Malabaricus, were continued.

#### ASTRONOMICAL SOCIETY.

The fifth Annual General Meeting of this Society was held on the 11th of February, for the purpose of receiving the report of the Council upon the state of the Society's affairs, electing Officers for the ensuing year, &c. Every lover of astronomy must be gratified to learn that the prosperity of the Society continues to increase; but the late period of the month at which we received the account of the proceedings precludes its insertion in the present number.

### ARTICLE X.

#### SCIENTIFIC NOTICES.

##### CHEMISTRY.

#### 1. On the Ingots of Copper obtained *viâ humidâ*.

By M. Clement.

THE beautiful experiments of Sir James Hall have demonstrated that pulverised carbonate of lime, a substance eminently decomposable by heat, may be fused, under great pressure, without losing its carbonic acid, and afford when cold a solid mass similar to marble.

In like manner as it was heretofore imagined that that mineral was necessarily formed by deposition from its aqueous solution, and could by no means be a product of heat, so at present it is generally believed that a solid mass of metallic copper capable of extension under the hammer, must have undergone igneous fusion, and have acquired its cohesion by cooling. Copper precipitated from its solution by whatever agent, is always in the state of a fine loose powder. The following fact, however, will show that an ingot of copper may be formed *viâ humidâ*. I am

indebted to M. Mollerat for the observation, which he communicated to me a short time since, on my visiting his fine manufactory for making vinegar from wood, in Burgundy.

In a series of operations for preparing sulphate of copper by calcining copper with sulphur, solutions of the sulphate are obtained, which become turbid by the separation of an insoluble subsulphate. They are placed in a tub, half buried in the ground, in order to become clear. It is against the interior sides of this tub, and always at the junction of two staves that small buttons (*champignons*) of metallic copper are observed to form, which gradually increase in size, and would doubtless ultimately become considerable masses. I have some specimens which I detached from the tub with a portion of the wood adhering to them.

On one side we find these bits of copper moulded on the wood of the tub, whose striae are impressed on their surface; on the other, they have the form of mammellæ with very minute, brilliant, crystalline facets.

One of these specimens weighs more than 75 grammes (nearly 2½ oz. English).

The chemical action by which the copper is revived is easily explained. The protosulphate of copper which unquestionably exists in the solution, in passing to the state of deutosulphate, deposits its base which gives up its oxygen and acid to form the new salt. It is evident that the revival of the copper may be effected in this manner without the assistance of any iron, and in fact there are no traces of that metal in the interior of the tub. It is not, however, this part of the phenomenon that appears to me most remarkable, but the cohesion, acquired by the copper so precipitated from the midst of a solution; a cohesion which is so great as to allow the metal to be hammered in the cold and reduced to thin leaves; and whose specific gravity is equal to that of fused copper, viz. 8.78. I have, moreover, filed a morsel of this copper, and have produced a surface as brilliant and free from pores, as could have been obtained by similar means with an ingot of common copper. (*Annales de Chimie*.)

## 2. Note on the Presence of Titanium in Mica. By M. Vauquelin.

M. Vauquelin, at the request of Mr. Peschier of Geneva (who conceived that he had found titanium in several micas in such quantity as to be an essential constituent of the mineral), repeated his experiments, first on two varieties of mica, and afterwards on many others, in all of which he detected the presence of titanium, but in very minute quantity, and in different proportions: the richest in titanium did not give more than one per cent. of that metal.

His mode of analysis was as follows:—He ignited the mica (divided into thin laminae, and cut very small with a pair of

scissors), with two parts of caustic potash, for half an hour, and digested the mass in 100 parts of water. Muriatic acid was gradually added to the mixture till it was slightly in excess; the solution evaporated slowly to dryness; the residuum washed with cold water, and the silica separated by the filter.

If the silica was coloured, which often happened, he digested it in cold muriatic acid diluted with 10 parts of water, till it became white; it was then washed, and while still moist boiled in strong muriatic acid. The liquid was then evaporated to expel the greater part of the acid, diluted and filtered, and the solution, containing only a slight excess of acid, treated with an infusion of galls. If titanium was present the solution first assumed a yellowish red colour, and soon afterwards tannate of titanium separated in flakes of the same colour.

The muriate of titanium is so easily decomposed by heat, that in general the greater part of the metal is found with the silica, which should always be carefully examined in all analyses in which titanium may be expected to be discovered. If, on the other hand, the evaporation have not been carried far enough, a portion may remain in solution in the washings of the silica. To be certain, precipitate the solution by ammonia, wash the precipitate, and digest it in caustic potash, which will dissolve the alumina, and the oxide of titanium, and the latter may then be separated by saturating the alkali with muriatic acid, and precipitation by infusion of galls.

Nearly two years since, I examined a dark brown mica, from Siberia, for titanium, without finding the least trace of that metal.—C.

#### MINERALOGY.

##### 3. *Harmotome*.

Dr. Wernekinck, of Giessen, has published a description and analysis of a new variety of this mineral in which the *barytes* is replaced by *lime*. Its constituents, according to his analysis, are :

Silica. ....	53.07
Alumina. ....	21.31
Lime. ....	6.67
Barytes. ....	0.39
Oxides of iron and manganese. ....	0.56
Water. ....	17.09
	<hr/>
	99.09

It is found in a basaltic amygdaloid, near the village of Anne-  
rode, at the distance of about a mile from Giessen. It always  
occurs in regular crystals, and the only crystalline form under  
which it has been hitherto observed is a perfect square prism,

terminated by four-sided acumination, whose sides rest upon the edges of the prism. The lateral planes of the prism are in every respect similar, and none of them are streaked. Sometimes these prisms are found combined together in the form of twin crystals.

The ordinary harmotome he found to be composed of

Silica. ....	44.79
Alumina. ....	19.28
Barytes. ....	17.59
Lime. ....	1.08
Oxides of iron and manganese. ....	0.85
Water. ....	15.32
	<hr/>
	98.91

The specimen which he analyzed was obtained from the Schifenberg, a hill in the neighbourhood of Giessen. Like the preceding, its crystal is a square prism, but it presents itself under a variety of modifications. Its matrix is also a basaltic amygdaloid.

The calcareous harmotome appears from his analysis to be a compound of 5 atoms of bisilicate of alumina + 1 atom of quadrosilicate of lime + 8 atoms of water. The barytic harmotome appears to contain a similar number of atoms of bisilicate of alumina and of water, combined with an atom of quadrosilicate or trisilicate of barytes.—(*Annalen der Physik.*)

#### 4. *Cadmiferous Sulphuret of Zinc.*

A specimen of common blende, of a reddish colour, passing into lead grey, was found by Dr. R. Brandes to be composed of

Sulphur. ....	33.838
Zinc. ....	58.150
Cadmium. ....	0.932
Iron. ....	7.028
	<hr/>
	100.548

(*Trommsdorff's Neues Journal der Pharmacie.*)

#### 5. *Sulphuret of Lead and Antimony.*

Trommsdorff found a specimen of this ore to be composed of

Sulphur. ....	20.9
Antimony. ....	22.4
Lead. ....	49.0
Iron. ....	4.0
Manganese. ....	2.0
Copper. ....	1.0
Loss. ....	0.7

100.0

The specimen had a colour intermediate between steel grey and lead grey, and an uneven fracture; it had no tendency to soil, and was moderately hard. Sp. gr. 7.477. Before the blowpipe it decrepitated strongly, and afterwards fused with great facility.—(Trommsdorff's Neues Journal der Pharmacie.)

#### MISCELLANEOUS.

##### 6. *The British Museum.—Mr. Goodwyn's Manuscripts.*

Those who are interested in mathematical computations, and the tabulation of their results for practical purposes, will learn with pleasure that the curious and extensive tables of the late *Henry Goodwyn*, Esq. of Blackheath, have, by the advice of Dr. Gregory, Professor of Mathematics in the Royal Military Academy, been deposited by Mr. Goodwyn's family in the library of the British Museum. The following copy of Dr. Gregory's account of the general nature of the manuscripts will serve to convey the requisite information to our readers.

The late Henry Goodwyn, Esq. of Blackheath, being for several years kept by ill health from the more active pursuits of life, devoted a great portion of his time to the most laborious computations, many of them relating to topics and leading to results that are exceedingly curious and interesting:—some to annuities; others, to the determination of powers and roots; several of these he applied to practical inquiries relative to interest, and others to the reduction and comparison of weights and measures, whether British or Foreign; and to the formation of a general system; and others he rendered applicable to the rules of mensuration, and to still higher inquiries among mathematicians.

In the pursuit of these researches, he developed various interesting properties, indicative of the mutual connection between circulating decimals and prime numbers entering either simply or compositely into the denominators of fractions respectively equivalent to those decimals; of which properties some have been long known to mathematicians, while others had almost, if not altogether, escaped their notice. A few of these are explained in the quarto appendix to the pamphlet to which this paper is attached; \* and, in that appendix, one of Mr. Goodwyn's ingenious improvements in computation is described and applied.

The results of his persevering and long-continued labours have, as yet, been only very partially laid before the public in a few detached pamphlets, volumes, &c., copies of all which are herewith transmitted: but, his two works of greatest labour, the one denominated *A Table of complete Decimal Quotients*; and the other, *A Tabular Series of Decimal Quotients for all the*

\* Entitled "The First Centenary of a Concise and Useful Table of Complete Decimal Quotients," with a Specimen of "A Tabular Series," &c.

*Proper Vulgar Fractions*, of which, when in their lowest terms, neither the numerator, nor the denominator is greater than 1,000, still remain in manuscript. The former of these is comprised in four folio volumes of manuscript, lettered *Table of Complete Quotients*. Mr. Goodwyn had finished their computation; and, by subsequent calculations, had nearly, if not entirely, verified the correctness of the whole. He had, also, advanced considerably, in the computation of the *Tabular Series*, the results being entered, and duly arranged, in five volumes large quarto; in the last of which, however, the *platform* of his labour is above exhibited. A comparison of the respective manuscripts with the two royal octavo printed volumes, entitled *Table of the Circles*, and *Tabular Series*, and with the quarto pamphlet, to which this is annexed, will enable any competent judge to appreciate the extent of these classes of Mr. Goodwyn's labours, their utility, and the comparative value of those portions which still remain unpublished.

Mr. Goodwyn's family, anxious to consign these manuscripts of their revered relative to some institution where they may be occasionally consulted by the friends and promoters of mathematical science, do now, with the consent of the trustees of the British Museum, deposit them in the library of that magnificent national institution.

Royal Military Academy,  
Woolwich, Nov. 1821.

OLINTHUS GREGORY.

### 7. Important Work on Conchology.

Messrs. Sowerby have recently issued a prospectus of a new work, which has long been wanting in this interesting branch of natural history. They propose to publish in quarterly numbers, descriptions, with coloured plates, of all the known species of recent shells. The first number will appear as soon as 100 subscribers shall have signified their intention of patronizing the work, which, from the acknowledged abilities of the authors, will (we have no doubt) very soon be done. The descriptions will be given by Mr. G. B. Sowerby in Latin and English, together with such observations as may be required, and the figures by Mr. J. D. C. Sowerby. The work will be printed on royal quarto, and each number will contain about 18 highly finished plates, coloured from nature, and comprise about 100 species; so that, when complete, there will be descriptions and figures of about 5,000 species. The authors are placed in circumstances peculiarly favourable to the production of a work of this kind, from the sale of the celebrated Tankerville collection having been entrusted to Mr. G. B. Sowerby, the possession of which, though necessarily only for a short time, will enable them to secure drawings and descriptions of many shells that could not otherwise be easily obtained. In addition to this,

the private collection of the authors, the immense number of species contained in the collection, late the property of Mr. George Humphrey, and the free access which the liberality of their friends allows to various other cabinets, will enable them to render the above work by far the most splendid and complete of its kind.

#### 8. *Electrical Conducting Power of Melted Resinous Bodies.*

It is commonly stated, that melted resins become good conductors of electricity and freely allow of its transmission. The following experiments were made with the view of determining to what extent they possess this property.

Common resin, shell lac, asphaltum, bees-wax, red and black sealing-wax were melted in separate glass tubes, fitted with wires for taking the electric spark: they all slowly and with difficulty drew off the charge of a jar, and not with the facility usually supposed. The melted contents of the same tubes acted as non-conductors when made part of the voltaic circuit.

Several thin glass tubes (previously tried by metallic coatings), were coated outside with copper foil, and about half filled with the melted substances, having wires dipping into them, similar to small leyden phials. The resinous coating, however, distributed no charge over the interior of the glass tubes, when connected with the machine, which would have been the case with conductors.

Upon removing the copper coatings and wires, substituting pointed wires bent at right angles, resting against the interior of the glass tube beneath the melted bodies, and suspending them successively from an electrical conductor, placing a metallic rod outside opposite the points, sparks passed in all cases perforating the glass.

The last cases would indicate that melted resinous bodies are not conductors, and the results obtained in the first instance may possibly be referred to heated air about the apparatus. T.G.  
—(Journal of Science.)

#### 9. *Lectures on the Phenomena and History of Igneous Meteors and Meteorites.*

E. W. Brayley, Jun. ALS., will shortly commence, at the Russel Institution, Great Coram-street, a Course of Lectures on the Phenomena and History of Igneous Meteors and Meteorites, illustrated by a series of transparent diagrams of Meteors, an extensive collection of Meteorites, and various experiments in Chemistry and Natural Philosophy.

# ARTICLE XI.

## NEW SCIENTIFIC BOOKS.

### PREPARING FOR PUBLICATION.

Principles of Political Economy and Population, including an Examination of Malthus's Essay. By John M'Iniscon.

A new and enlarged edition of Dr. Prout's Inquiry into the Nature and Treatment of Calculus, &c. will shortly be ready for publication.

The Present State of the Mines in Mexico, Chile, Peru, and Brazil. 12mo.

Practical Chemical Mineralogy. By Frederick Joyce.

A Voyage in 1822, 1823, 1824, containing an Examination of the Antarctic Sea, to the Seventy-fourth Degree of Latitude; and a Visit to Terra del Fuego. By James Weddell. 8vo.

### JUST PUBLISHED.

A Lecture on the Origin, Progress, &c. of Shipping and Commerce, delivered at the Bristol Philosophical and Literary Society. By Charles Pope, Esq. 1s. 6d.

A System of Pathological and Operative Surgery. By Robert Allan, FRSE. &c. Vol. 3.

Evils of Quarantine Laws and Non-existence of Pestilential Contagion. By Charles Maclean, MD. Second Edition. 8vo. 15s.

Shaw's General Zoology. Vol. 12.

The Botanic Garden, or Magazine of Hardy Flower Plants. By B. Maund. Nos. I. and II. with four coloured Figures, post 4to. 1l. 6s. foolscap 4to. 1l.

Unwin's Companion of Medicine. 8vo. 7s. 6d.

Parry's Pathology. Vol. 1. 8vo. 14s.

Testimonies in favour of Salt as a Manure, and as a Condiment for the Horse, Cow, and Sheep. By the Rev. B. Dacre, ALS. 8vo. 6s.

Narrative of the Unsuccessful Attempt to reach Repulse Bay. With a Chart, and Engravings by Finden. By Capt. Lyon, RN. 10s. 6d.

Observations on a General Iron Railway, or Land Steam Conveyance, &c. By Thomas Gray. Fifth Edition, enlarged. 8vo. 8s. 6d.

Cards of Euclid. By the Rev. J. Brasse. 5s. 6d. in a Case.

# ARTICLE XII.

## NEW PATENTS.

C. Heathorn, Maidstone, lime-burner, for his method of constructing and erecting a furnace, or kiln, for the more speedy, more effectually, and more economically manufacturing of lime, by means of applying, directing, and limiting, or regulating the flame and heat arising in the manufacturing or burning coal into coke, and thus making lime and coke in one and the same building, and at one and the same time.—Nov. 11.

W. Leathy, Great Guildford-street, Southwark, engineer, for improvements in the machinery used in making bricks, and improvements in drying bricks, by means of flues and steam.—Nov. 11.

P. Brunet, Wimpole-street, Cavendish-square, merchant, for a furnace made upon a new construction.—Nov. 11.

J. C. Daniell, Stoke, Wilts, clothier, for improvements in dressing woollen cloth.—Nov. 20.

I. Taylor, jun. Chipping Ongar, Essex, for a cock or tap for drawing off liquids.—Nov. 20.

W. Rhodes, Hoxton, brick-maker, for his improvement in the construction of clauaps for burning raw bricks.—Nov. 20.

L. Lambert, Cannon-street, for improvements in the material and manufacture of paper.—Nov. 23.

S. Wilson, Streatham, Surrey, for a new manufacture of stuffs with transparent and coloured figures.—Nov. 25.

W. S. Burnett, New London-street, merchant, for improvements in ships' tackle.—Nov. 25.

J. Osbaldeston, Shire Brow within Blackburn, Lancashire, calico-weaver, for his improved method of making healds to be made in the weaving of cotton, silk, woollen, and other cloths.—Nov. 29.

T. Hancock, Goswell Mews, Goswell-street, patent cock manufacturer, for his method of making or manufacturing an article which may be in many instances substituted for leather, and be applied to other useful purposes.—Nov. 29.

W. Furnival, Anderton, Cheshire, salt manufacturer, for improvements in the manufacture of salt.—Dec. 4.

W. W. Young, Newton Nottage, Glamorganshire, engineer, for improvements in manufacturing salt, part of which improvements are applicable to other useful purposes.—Dec. 4.

J. H. Suwerkrop, Vine-street, Minorics, merchant, for an apparatus or machine, which he denominates "A thermophore, or a portable-mineral or river-water bath and linen warmer;" and also for other apparatus or machines connected therewith for filtering and heating water.—Dec. 4.

G. Wycherley, Whitechurch, Salop, saddler, for improvements in making and constructing saddles, and side-saddles.—Dec. 4.

R. Dickenson, Park-street, Southwark, Surrey, for his improved air-chamber for various purposes.—Dec. 7.

J. Thompson, Pembroke-place, Piccadilly, for his improved mode of making cast-steel.—Dec. 9.

R. Bowman, Aberdeen, chain-cable maker, for his improved apparatus for stopping, releasing, and regulating chain and other cables of vessels.—Dec. 9.

W. Moulton, Lambeth, engineer, for his improvement in working water-wheels.—Dec. 9.

Sir W. Congreve, Cecil-street, Strand, for his improved gas-meter.—Dec. 14.

S. Davis, Upper East Smithfield, gun-lock-maker, for improvements in guns and other fire-arms.—Dec. 18.

D. Gordon, Basinghall-street, London, for improvements in carriages or other machines to be moved or propelled by mechanical means.—Dec. 18.

S. Roberts, Parke Grange, near Sheffield, silver-plater, for his improvement in the manufacture of plated goods of various descriptions.—Dec. 18.

P. J. B. V. Gosset, Clerkenwell Green, for improvements in looms or machinery for weaving various sorts of cloths or fabrics.—Dec. 18.

J. Gardner, smith, and J. Herbert, carpenter, both of St. Leonards, Gloucestershire, for improvements on machines for shearing or cropping woollen cloths.—Dec. 18.

W. F. Snowden, Oxford-street, machinist, for his invented wheel-way and its carriage for the conveyance of passengers, merchandize, and other things along roads, rail, and other ways, either on a level or inclined plane, and applicable to other purposes.—Dec. 18.

J. Weiss, Strand, surgical-instrument maker and cutler, for improvements on exhausting, injecting, or condensing pumps or springs, and on the apparatus connected therewith, which improvements are applicable to various useful purposes.—Dec. 18.

J. and W. H. Deykin, Birmingham, button-makers, for an improvement in the manufacture of military and livery buttons.—Dec. 23.

D. Stafford, Liverpool, for improvements on carriages.—Dec. 24.

S. Denison, Leeds, whitesmith, and J. Harris, Leeds, paper-mould-maker, for improvements in machinery for the purpose of making wove and laid paper.—Jan. 1, 1825.

P. Erard, Great Marlborough-street, musical-instrument maker, for improvements in piano-fortes.—Jan. 5.

A. Tilloch, LL.D. of Islington, for improvements in the steam-engine or apparatus connected therewith.—Jan. 11.

W. Henson and W. Jackson, Worcester, lace-manufacturers, for improvements in machinery for making bobbin-net.—Jan. 11.

G. Gurney, Argyle-street, Hanover-square, surgeon, for his improved finger-keyed musical instrument, in the use of which a performer is enabled to hold or prolong the notes, and to increase or modify the tone.—Jan. 11.

F. G. Spilsbury, Leek, Staffordshire, silk-manufacturer, for improvements in weaving.—Jan. 11.

W. Hirst, Leeds, cloth-manufacturer, for improvements in spinning and shabbing machines.—Jan. 11.

J. F. Smith, Dunston Hall, Chesterfield, for improvements in the preparation of slivers or tops from wool, cotton, or other fibrous materials.—Jan. 11.

J. F. Smith, Dunston Hall, Chesterfield, for improvements in dressing and finishing woollen cloths.—Jan. 11.

J. F. Atlee, Marchwood, Southampton, for a process by which planks and other scantlings of wood will be prevented from shrinking, and will be altered and materially improved in their durability, closeness of grain, and power of resisting moisture, so as to render the same better adapted for ship-building and other building purposes, for furniture and other purposes where close or compact wood is desirable; inasmuch that the wood so prepared will become a new article of commerce and manufacture, which he intends calling "condensed wood."—Jan. 11.

G. Sayner, Hunslet, Yorkshire, dyer, and J. Greenwood, Gomersall, in the said county, machine-maker, for improvements in the mode of sawing wood by machinery.—Jan. 11.

T. Magrath, Dublin, for his composition to preserve animal and vegetable substances.—Jan. 11.

T. Magrath, Dublin, for his improved apparatus for conducting and containing water and other fluids, and preserving the same from the effects of frost.—Jan. 11.

J. Phipps, Upper Thames-street, stationer, and C. Phipps, River, Kent, paper-maker, for improvements in machinery for making paper.—Jan. 11.

W. S. Burnet, London-street, for a new method of lessening the drift of ships at sea, and protecting them in gales of wind.—Jan. 11.

J. Andrew, G. Farlton, and J. Shepley, Crumshall, near Manchester, cotton-spinners, for improvements in the machine used for throstle and water spinning of thread or yarn, which improved machine is so constructed as to perform the operations of sizing and twisting in or otherwise removing the superfluous fibres, and of preparing a roving for the same.—Jan. 11.

J. Heathcoat, Tiverton, lace-manufacturer, for improvements in machinery for making bobbin-net.—Jan. 12.

W. Booth and M. Bailey, Congleton, Cheshire, machinists, for improvements in spinning, doubling, throwing, and twisting silk, wool, cotton, flax, &c.—Jan. 13.

J. Lockett, Manchester, engraver to calico-printers, and copper-roller manufacturer, for improvements in producing a neb or slob in the shell or cylinder, made of copper or other metal, used in the printing of calico.—Jan. 14.

W. Rudder, Eghaston, Birmingham, cock-founder, for improvements in cocks.—Jan. 18.

W. Church, Birmingham, for improvements in casting cylinders, tubes, and other articles of iron and other metals.—Jan. 18.

F. Melville, Argyle-street, Glasgow, piano-forte maker, for his improved method of securing the small piano-fortes from the injuries to which they are liable from the tension of the strings.—Jan. 18.

E. Lees and G. Harrison, brick-makers, Little Thurrock, Essex, for an improved method of making bricks, tiles, &c.—Feb. 1.

J. Thin, Edinburgh, architect, for a method of constructing a roasting jack.—Feb. 1.

S. Crosley, Cottage-lane, City-road, for certain apparatus for measuring and registering the quantity of liquids passing from one place to another.—Feb. 1.

S. Crosley, Cottage-lane, City-road, for an improvement in the construction of gas regulators of governors.—Feb. 1.

T. Burstall, Bankside, Southwark, and J. Hill, Greenwich, engineers, for a locomotive of steam-carriage.—Feb. 3.

G. Augustus Lamb, DD. Rye, Sussex, for a new composition of malt and hops.—Feb. 10.

R. Badwall, jun. Leek, Staffordshire, silk-manufacturer, for improvements in the winding, doubling, spinning, throwing, or twisting of silk, wool, cotton, &c.—Feb. 10.

J. Heathcoat, Tiverton, Devonshire, lace-manufacturer, for improvements on the method of manufacturing silk.—Feb. 11.

## ARTICLE XIII.

## METEOROLOGICAL TABLE.

1825.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.
		Max.	Min.	Max.	Min.		
1st Mon.							
Jan. 1	W	30.23	30.15	54	42	—	05
2	W	30.48	30.15	45	35	—	—
3	S W	30.48	30.11	54	36	—	16
4	W	30.66	30.11	53	32	—	—
5	N	30.78	30.66	37	25	—	—
6	N	30.78	30.65	49	25	—	—
7	W	30.79	30.65	45	32	—	—
8	N	30.89	30.79	42	32	—	—
9	N W	30.89	30.89	42	38	—	—
10	N	30.89	30.86	50	35	—	—
11	N W	30.86	30.80	42	29	—	—
12	N W	30.80	30.70	48	32	—	—
13	N W	30.70	30.53	50	37	.47	—
14	W	30.53	30.41	42	39	—	—
15	W	30.41	30.02	42	32	—	—
16	S W	30.04	30.02	43	31	—	07
17	N W	30.04	29.62	46	37	—	03
18	S	29.65	29.62	42	30	—	35
19	S W	29.76	29.63	39	31	—	—
20	N W	29.92	29.76	42	36	—	06
21	N E	30.10	29.92	43	32	—	—
22	N E	30.34	30.10	41	32	—	—
23	N	30.34	30.23	40	31	—	—
24	N	30.23	29.85	40	32	.45	15
25	N W	30.20	29.85	40	27	—	—
26	S	30.21	30.20	40	32	—	07
27	S W	30.68	30.21	51	32	—	—
28	N	30.85	30.68	44	24	—	—
29	Var.	30.84	30.64	38	24	—	—
30	W	30.64	30.60	45	38	—	—
31	W	30.60	30.27	50	44	—	01
		30.89	29.62	54	24	.92	.95

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

## REMARKS.

*First Month.*—1. Fine. 2—4. Cloudy. 5. Fine. 6. White frost, and foggy morning: fine day. 7. Cloudy. 8. Very fine day. 9. Cloudy. 10. Overcast. 11. Ditto. 12. Foggy: gloomy. 13, 14. Gloomy. 15. Gloomy: fine afternoon. 16. Overcast: showery. 17. Fine. 18. Rainy morning: wind high: rainy. 19. Fine. 20. Fine: some rain at night. 21, 22. Cloudy. 23. Cloudy: cold. 24. Cloudy. 25. Fine. 26. Drizzly. 27. Fine. 28. Very fine. 29. Hoar frost and fog. 30. Cloudy. 31. Cloudy.

## RESULTS.

Winds: N, 7; NE, 2; S, 2; SW, 4; W, 8; NW, 7; Var. 1.

Barometer: Mean height

For the month, ..... 30.376 inches.

For the lunar period, ending the 11th, ..... 30.240

For 13 days, ending the 8th (moon north) ..... 30.401

For 14 days, ending the 22d (moon south) ..... 30.279

Thermometer: Mean height

For the month, ..... 38.396°

For the lunar period, ..... 42.258

For 29 days, the sun in Capricorn, ..... 39.551

Evaporation, ..... 0.92 in.

Rain ..... 0.25

# ANNALS OF PHILOSOPHY.

APRIL, 1825.

## ARTICLE I.

*On the Origin of Alluvial and Diluvial Formations.*  
By Professor Sedgwick.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

Trinity College, Cambridge, March 11, 1825.

THE existence of widely extended masses of incoherent materials separating the vegetable soil from the solid strata of the earth, is a fact which forces itself upon the attention of every practical geologist. These materials have for many years been divided into two classes. The first composed of a series of deposits originating in such causes as are now in daily action. The second composed of various materials irregularly heaped together, often transported from considerable distances, and supposed to have originated in some great irregular inundation. Since the publication of Cuvier's great work on fossil quadrupeds, this distinction has been very generally admitted; especially as it seemed to be completely borne out by the zoological phenomena exhibited by the two separate classes of deposits.

Prof. Buckland was, I believe, the first geologist who adopted the terms *diluvium* and *alluvium*, *diluvial detritus* and *post-diluvial detritus* to designate the two classes of phenomena above alluded to. The propriety of this separation has been since confirmed by a long series of well-conducted observations; and by the interesting discoveries brought to light by the same author within the last four years, some important errors have been corrected, and the whole subject has assumed a form and a consistency which it unquestionably never had before. Since the publication of the "*Reliquiæ Diluvianæ*," many objections have been urged against the opinions advanced in that work. The greater part of the objectors are undeserving of any animadversion, as they appear entirely ignorant of the very elements of geology, and far too imperfectly acquainted with the facts about which they write to have it in their power to turn them to any

account, or to draw a single just conclusion from them. This censure does not, however, apply equally to them all. A writer in the two last numbers of the Edinburgh Philosophical Journal considers the present classification of the superficial *detritus* of the earth to be founded on an imperfect induction, and to be contradicted, or at least invalidated, by the distribution of the organic remains contained in it. Though I am opposed to many of the conclusions of this author, and think that he has been misled from a want of a more extended knowledge of the phenomena in question, yet I willingly allow that his arguments are adduced with a sincere love of truth, and that his facts and inferences are entitled to a candid examination. It is not, however, my intention formally to enter the field of controversy. Prof. Buckland is far too secure in his position, and incomparably too well armed to need any such assistance.

The words *alluvial* and *diluvial detritus* designate certain classes of phenomena which at the same time have a distinct character, and belong to distinct epochs. The propriety of this assumption can only be made out by direct observation. If it appear that *alluvial* formations commonly rest on *diluvial*; that the converse is never true; and that the two formations never alternate: then the distinction just alluded to is completely made out, and rests on exactly the same evidence as the order of superposition of any known strata. We may further observe that this conclusion is quite independent of any zoological arrangements. When the order of superposition has been once made out, we may then proceed to examine the zoological phenomena of each successive deposit. Before that time, organic remains, however interesting in themselves, convey little information respecting the revolutions to which the earth's surface has been subjected. It has been already observed that the words *diluvial detritus* were applied to certain materials brought into their present situation by great irregular inundations. In what sense all *diluvial* formations may be considered contemporaneous; to what extent, and in what manner, diluvial torrents have acted on the earth's surface, are simply questions of fact to be determined by physical evidence, and by physical evidence alone.

The truth of any physical phenomenon can only be made out by physical evidence, and no appeal ought to be made to any other authority before that evidence has been completely investigated. It is then obvious that every conclusion respecting the classification of formations, of whatever age, can only rest on the evidence afforded by direct observations. For this reason, I have drawn up, for insertion in the *Annals of Philosophy*, an account of some of the *alluvial* and *diluvial* deposits which I have had an opportunity of personally examining. Part of the succeeding statements may be considered unnecessary, and

some of the facts may be thought too unimportant to deserve any notice. If, however, they should throw any light on a disputed subject, or should they in any way strengthen the chain of evidence by which one of the most important inductions of geology has been established, they will not be altogether without their use. I have the honour to be, Gentlemen,

Your most faithful servant,

A. SEDGWICK.

#### SECT. I. — *Alluvial Deposits.*

All the principal vallies of England exhibit in their higher portions occasional examples of nearly horizontal deposits of comminuted gravel, silt, loam, and other materials accumulated by successive partial inundations. The nature of these *alluvial* deposits and the cause of them are so obvious, that it is unnecessary to refer to particular instances. If we descend from the hilly and mountainous regions, and examine the courses of our rivers near their entrance into any widely extended plains, we frequently find their banks composed of incoherent materials of a new character. They are not made up of thin layers of comminuted matter formed by successive inundations, or of silt and turf-bog accumulated in stagnant waters, but of great irregular masses of sand, loam, and coarse gravel, containing through its mass rounded blocks sometimes of enormous magnitude. It is at once evident that the propelling force of the river is entirely inadequate to the transport of such materials as these. We may observe, moreover, that they are not confined to the banks of the rivers, but spread over all the face of the country, and often appear at elevations many hundred feet above the level of any natural inundation. To such materials as these the term *diluvial* (indicating their formation by some great irregular inundation) is now applied by almost all the English school of geologists.

The rivers which descend from the western moors and unite in the great central plain of Yorkshire, afford a succession of beautiful illustrations of the appearances which have been just described. While rolling from the mountain chain, and uniting with their different tributary branches, they leave masses of alluvial matter in every place where the form of the valley admits of such a deposit: and after passing through the inferior region and escaping through many ravines and gorges into the great plain of the new red sandstone, they then find their way through enormous masses of *diluvial debris* which often mask the inferior strata through considerably extended tracts of country. If we follow any of these rivers into the central parts of the great plain, we may still find (with occasional interruptions) the *diluvial debris* descending with the surface of the

ground, often forming the channel of the waters, and, where the level of the country admits of it, sometimes surmounted by an accumulation of newer *alluvial* materials. By the ordinary action of the waters, the two distinct classes of deposits sometimes become mixed and confounded; but I have never seen an example where their order is inverted, or where, through any extent of country, they alternate with each other. The instances adduced are not exceptions to, but examples of, the general rule. There is not, I believe, a single river in England which does not afford a more or less perfect illustration of some of the phenomena above described.

Perhaps the most important class of facts connected with *alluvial* phenomena, and which at the same time very strikingly exhibit their relation to all other deposits in this country, are to be met with in the low marshy regions near the mouths of some of our larger rivers. In proof of this assertion I shall proceed to describe some of the physical characters of the fenny tract of country which stretches from the south part of Lincolnshire to the base of the chalk hills of Norfolk, Suffolk, and Cambridgeshire. If a section be made through this region in a direction which is transverse to the outfall of the waters, its profile will be represented, first, by a line descending from the higher part of Lincolnshire to the level of the fens; secondly, by a succession of horizontal lines exhibiting the several levels of distinct fenny regions, interrupted here and there by extensive protuberances of *diluvial* gravel;\* lastly, by an undulating line ascending from the *alluvial* region to the top of the hills which form its south-eastern boundary. If a section were made in a direction transverse to the former, commencing at the south-west boundary of the low lands, and ending in the sea, its profile would be represented, first, by a line showing the descent of the high lands to the level of the fens; secondly, by a long line extending almost at a *dead level* (except where it is interrupted by some of the protuberances above-mentioned) to the eastern extremity of the fens in the immediate vicinity of the coast; lastly, by a line *descending rapidly* from the level of the fens to low water mark.† The singular contour indicated by the second section has unquestionably arisen from the continued accumulation of *alluvial* silt which has choked up the mouths of the rivers, and raised their beds and all the contiguous country far above their ancient level.‡

\* During great inundations these diluvial hills resemble islands rising out of an inland sea. Most of the towns and villages in the Isle of Ely are built upon them.

† Thus from Peterborough to Sutton Wash below Wisbeach (a distance of more than twenty miles), the fall of the water is on the average three inches and a half for each mile. But from Sutton Wash to low-water-mark at Crabhole, the fall is more than three feet for each mile.—(See Rennie's Report on the Drainage of the Bedford Level.)

‡ Thus Thorny Earth fen is thirteen feet; Peterborough low fen twelve feet six

It is not, however, the external contour so much as the internal structure of the district, which bears on the subject of this paper. The whole of the alluvial *delta* exhibits, as might be expected, a great uniformity in the arrangement of its constituent beds. When the vegetable coating is removed from any part of it, we may generally find below a brownish black earth which is formed of a variable mixture of common vegetable soil, of peat, and of alluvial silt. The different qualities of fen land arise out of the variable proportions of these constituents. In those tracts which are pent up between high artificial banks and upon which water frequently stagnates, the soil is almost exclusively composed of decayed vegetable matter converted more or less perfectly into the state of peat. In other more favoured tracts, more especially on the sloping skirts of the diluvial hills, the soil is of great fertility, and is composed principally of the accumulated silt of successive inundations. Materials possessing some of these characters are in many places accumulated upon the regular strata of the country to the thickness of nearly twenty feet. When they are laid bare by any artificial section, we may often see various modifications which are so far interesting as they throw light upon the ancient history of these deposits. In one part of such a section we may find the prevailing black earth interrupted by thin beds of peat, each of which indicates the temporary residence of stagnant water. In another part of the same section, the prevailing soil is seen to alternate with layers of sand and silt which mark the effects produced by extraordinary land floods. Alternations like these are so common as hardly to deserve any notice. If the section descend still further, we not unfrequently find the whole series of alluvial deposits separated from the true substratum (which in many places is composed of a stiff blue clay)\* by a very thin bed of light coloured, unctuous, marly silt. This marly silt is, if I mistake not, of great antiquity, and must have been deposited by the waters prior to the existence of any portion of the alluvial covering.

If all the soil and accumulated *détritus* were removed from the district I am considering, it is certain that the surface of the ground would present many considerable irregularities. It is further evident that such a surface must in ancient times have

inches; Peterborough great fen thirteen feet two inches above the level which ought to form the base of the drainage near the sea.—(See Bower's Report of the New Drainage near Boston.)

\* In all the central parts of the fens, the blue substratum contains innumerable specimens of the characteristic *gryphaa dilatata* of the Oxford clay; but near Ely, under the alluvial and diluvial *détritus*, there is a bed which contains the *ostrea deltoides* of the Kimmeridge clay. If I mistake not, the coral-rug formation thins off before it reaches the fens, where the two clays are probably brought into immediate contact. Below Cambridge, the tracts of fen land rest on the gault or Folkestone clay.

supported many varieties of productions which are now so deeply buried as to be reached only by occasional artificial excavations. This remark at once explains the variable thickness of different portions of the fen lands, and the extraordinary appearances we sometimes meet with in digging through them. For example: in excavating the foundations of the new *lock* on the river Cam between Clay Hithe and Ely, they reached (after passing through ten or twelve feet of common fen soil) a bed of considerable thickness composed almost entirely of hazel wood and hazel nuts. The wood could not, I think, have been drifted from any great distance; and the enormous accumulation of nuts (many pecks of which might have been collected in the space of a few square yards) seemed to be the production of an ancient period when, year after year, the trees shed their fruit on the ground, and there were no inhabitants to collect it. In many other places, after passing through a thick coating of turf bog and alluvial silt, we meet with the branches, trunks, and even the roots of large timber trees. Some of these may have been floated down during great floods from the neighbouring high lands; but the far greater number of them have unquestionably grown near the spots where we now find them. Examples of this kind are, I believe, supplied by almost all the extensive fen regions in our island.

Lastly, I shall briefly notice a class of facts which, although admitting of a very easy explanation, have sometimes led to erroneous conclusions. In almost all the marsh lands which border on the sea, the *alluvium* is separated from the old subjacent strata by a quantity of marine silt, and sometimes by beds of sea shells which appear to have lived and died on the spot where they are now found. The extent of this marine deposit towards the interior of the country plainly indicates the extent to which the alluvial materials have been accumulated and pushed down within the ancient line of the sea coast. But the case is not always as simple as I have here stated it. The lower portion of the marsh lands in question sometimes exhibit several distinct alternations of marine silt and shells, with turf bog and other freshwater deposits. Facts of this kind were (if I have not been misinformed) observed in some of the lower parts of the Eau Brink cut which was lately completed in the neighbourhood of Lynn. We are not to suppose that such facts indicate any sudden change in the relative level of land and sea. All the alternations above described are below the level of high-water, and naturally result from the manner in which the fen lands have been formed. We have only to recollect that in the places alluded to, the tides have for many ages been ebbing and flowing along a system of planes which have been perpetually encroaching on the coast, and perpetually

changing their inclination. Of such a state of things, the occasional admixture of marine and freshwater deposits, and the occasional alteration is the inevitable consequence.\*

### SECT. 2.—*Diluvial Formations.*

It remains for me briefly to notice the diluvial formations which appear within the limits of the tract I have been describing. They seem to have been rapidly and irregularly accumulated by an inundation which acted with extraordinary violence; for they are partly composed of broken masses of more ancient strata, which are rounded and ground down by attrition, and which in many instances have been transported from distant parts of the country; and they contain no alternations indicating (as in the case of alluvial deposits) the *long continued and tranquil* operation of the agents by which they have been produced. They rest on the ancient strata of the country without the intervention of any other deposit whatsoever, and in instances without number they form the basis of the whole alluvial detritus.

The true relations of the *diluvial detritus* are beautifully exemplified on the flanks of the chalk hills which skirt the south-eastern side of the marsh lands above described. It is constantly seen to rest immediately on the fundamental rock; to follow all the irregularities of the surface; to rise out from beneath all the alluvial lands, and sometimes to lie in scattered masses on the very top of the chalk downs. From thence it may be traced, almost in a continuous mass, still further to the south-east, where it is heaped up to an enormous thickness, and overlies the newest *tertiary* beds which exist in that part of England. From all these facts we are justified in concluding, that the *diluvial* and *alluvial* deposits above described are not only essentially different in their structure, but belong to two distinct epochs; the former class of deposits having been produced by some extraordinary disturbing forces prior to the existence of any portion of the other class.

Were the order above given contradicted by the arrangements of the superficial deposits in other parts of our island, we should of course be prevented from drawing any general conclusion from it. But I believe there is no inconsistency in the order of our superficial deposits, and that the counterpart of the

\* A fine instance of this kind of alternation may be seen in the lower Pentowan stream-work near St. Austle. The *diluvial tin ground* (which is nearly thirty feet below the level of high-water) is covered with a deposit (about seven feet thick) of compressed vegetable matter, leaves, roots, and trunks of trees, &c. all of which have evidently been drifted into their present position by floods, or perhaps by the slide of a half-formed turf bog. Over this deposit are a succession of marine beds (above twenty feet thick) obviously accumulated, while the lower part of the valley was an estuary. Lastly, a thick formation of peat, containing branches and trunks of trees, rises above the level of high-water, and is surmounted by common vegetable soil. For a detailed account of this section, see Geol. Trans. vol. i. p. 494, &c.

facts above stated may be found in every country which is similarly circumstanced with that which has been described. My object is not, however, to make out a new arrangement, but to confirm an old one; I shall, therefore, content myself with referring to one additional class of examples.

In many parts of Cornwall the flanks of the central chain of hills are covered with a thick deposit of diluvial gravel, which, after resting immediately on the granitic and schistose rocks of the country, and following their inclination, often descends into the lower part of the transverse valleys, and from thence shelves down below the level of the sea. Near the mouths of these valleys the *diluvium* is always covered up by beds of a more recent *detritus* which in some places are nearly sixty feet thick. Notwithstanding their great thickness, many large excavations have been made through them for the purpose of extracting the tin ore which has been washed down from the mountains at the time the *diluvial* rubbish was formed, and which (in consequence of its great specific gravity) has naturally subsided to the bottom of the formation. In various excavations of this kind (provincially called stream-works), conducted in different parts of the county, we may see in the clearest manner the true relations of the several superficial deposits; and (as far as any thing can be proved by single instances) the sections show; first, that all the *diluvial detritus* in that part of England originated in the same system of causes which, having produced their effects once, were never repeated; \* secondly, that all the *alluvial detritus*, of whatever kind, is posterior to the preceding; because it constantly rests upon it, and never alternates with it.

By the examination of facts like these, we become acquainted with the natural history of such superficial deposits as I have been describing. The facts are in strict accordance with every thing which I have myself observed, and they are, I believe, in accordance with the observations of all English geologists who have personally examined the evidence connected with this subject. We may therefore conclude on an induction founded on a very wide range of consistent observations; 1. That *alluvial deposits* include a large class of formations which have originated in causes such as are now in daily action; 2. That the same causes have acted during a long period; 3. That during that period the deposits have not been interrupted by any catastrophe which has interposed any other deposits of a distinct character; 4. That *diluvial deposits* possess a distinct character from the preceding class, never alternate with them, and, from their position, evidently belong to an older epoch; 5. That during the epoch in question, the *diluvial* gravel was produced by extraor-

\* This fact is of great importance and was, I believe, first remarked by Townsend in his "Vindication of Moses." (See vol. i. p. 227, &c.) I had repeated opportunities of verifying this remark during a tour in Cornwall made in the summer of 1849.

dinary inundations; 6. That the disturbing forces which produced these inundations acted on the earth's surface after the deposition of all the regular strata with which we are acquainted.

The separation of the incoherent materials, which are heaped on the regular strata of the earth, into *diluvial* and *post-diluvial detritus*, is, therefore, a natural separation, which is at once descriptive of the things designated, and founded on the constant relations which they bear to each other. Moreover it is unconnected with any hypothesis whatsoever, and is independent of any argument drawn from the nature of the organic remains contained in different parts of the several deposits.

### SECT. 3.—*Organic Remains in Alluvial Formations.*

I should not have dwelt so long in illustrating the preceding conclusions, had I not known that the nature of the evidence on which they are founded has often been entirely overlooked or misunderstood. In the next place, I shall briefly consider the organic remains contained in the two classes of deposits, especially in those localities which have been already described. The following specimens were derived from the *alluvial debris* which rests on the *diluvial tin ground* in various parts of Cornwall.

1. A human skull buried 36 feet in *alluvium*, from the Canon *stream-work*. 2. Horn of an ox 40 feet deep in *alluvium*, from the same place. 3. Fragments of a human skeleton, from the Pentowan *stream-work*. 4. An ancient earthen vessel, formed without the potter's wheel, more than 40 feet deep in *alluvium*, and about 10 or 12 feet *above the diluvial tin ground*, from the same place. 5. Part of a culinary vessel buried 24 feet in *alluvium*, from the Lanyon *stream-work*. A celt and some other rude works of art were found near the same place. To the preceding might be added a long list of spoils derived from the *alluvial* region which stretches out from the neighbourhood of Cambridge to the wolds of Lincolnshire, such as various specimens of trunks and branches of trees, of freshwater and land shells; of implements of human workmanship; of horns, teeth, and sometimes skeletons of animals which have been either drifted into the marshes, or have perished there by accident or violence, &c. &c. To which catalogue might be added, the skeletons of four beavers found near Chatteris in the *alluvial* bed of the Old West-water, a river which in former times performed an important part in the drainage of the country, but which has been choked up for 200 or 300 years.\* We look in vain into these lists for the bones of the cavern-bear, the mam-

\* See a paper by John O'Sha, Esq. in the Transactions of the Cambridge Philosophical Society, vol. i. p. 175.

moth, the hyæna, the rhinoceros, the hippopotamus, and other animals, the spoils of which are found in almost miraculous abundance in many parts of the world buried in the old *diluvial detritus*.

When we consider the great extent of the *alluvial* tract above-described, and the various cuts and drains which have been made through almost every part of it: and when we further consider that the same tract of country is the growth and accumulation of at least 2000 or 3000 years; the negative argument becomes complete, and we conclude, almost with certainty, that during this long period not one of the several species of animals last enumerated existed in the neighbouring parts of our island. Let these considerations be combined with the admirable details and illustrations supplied in the writings of Cuvier and Buckland, and we readily extend the same conclusion to other parts of England, and indeed to every part of the world, which has been rigidly examined.

It may, however, be urged that no accumulation of negative evidence can stand against the direct evidence of opposing facts. Is then the preceding conclusion opposed by any incontrovertible facts? To such a question I should not hesitate to reply by a decided negative. Ambiguous cases may occur near the base of a crumbling sea cliff, or near the bank of a river which is continually falling down from being undermined by the attrition of the waters; or in the silt and alluvial rubbish of a valley which for many ages has been modified and ravaged by successive floods. In such situations the spoils of *alluvial* and *diluvial* deposits may be *mechanically mixed* together so as to render it impossible to separate them.

A sober-minded naturalist who makes his inductions after an extended examination of facts, and who does not view all things through the distorting medium of an hypothesis, will never derive from such localities as these any argument for the true arrangement of spoils found in different parts of the superficial gravel. The only way in which spoils derived from such situations can be classified, is by comparing them with similar remains found in other deposits, the relations of which are clearly exhibited, and which have been modified by no subsequent disturbing forces. Had this observation suggested itself to Dr. Fleming, he might have withheld more than half the examples he has brought forward in the Edinburgh Philosophical Journal (No. 22, p. 297, &c.) with a view of overturning the distinction which has been drawn between the organic remains of *alluvial* and *diluvial detritus*. In regard to the mammoth, he has not produced a single example of its remains found in undisturbed *alluvium*. Some of his examples may, perhaps, be ambiguous; but others are derived from localities which, had he taken the trouble to examine them himself, he would

have known to be *diluvial*. A single example is given of the bones of the hippopotamus found *under* a peat-bog. But the fact is given without details, and without the shadow of a proof that the bones were buried in *alluvium*. The case of the great fossil elk may perhaps be ambiguous. A gigantic animal of that family would soon be marked out for destruction; or it may perhaps have been exterminated by beasts of prey before the peopling of Western Europe. All the spoils of this creature which I have myself seen *in situ* do, however, belong to *diluvial* deposits. The three examples of horns of the rhinoceros found in *alluvial* marl-pits and turf-bogs, and preserved in the museum of Edinburgh, seem at first sight to throw most formidable difficulties in the way of the received classification. Through the kindness of Prof. Jameson, I have lately seen the specimens in question, and I know from the Professor himself that there is no adequate evidence to prove them genuine fossils. Without this information, from their look and their condition, I should not have hesitated a moment in rejecting them as spurious. It is contrary to my present object to enter into any details connected with the examples to which I have referred. I do, however, unhesitatingly assert, that as far as regards the purpose for which they were adduced, they are altogether without weight, and without importance.

#### SECT. 4.—*Organic Remains in Diluvial Detritus.*

The *diluvium* in the central parts of the fens of Cambridge-shire, or on the skirts of the low hills by which the region is skirted, is found to possess a great uniformity of character. It contains innumerable fragments of gryphites, echinites, shells, corals, lizards' bones, and other fossils, all more or less perfectly mineralized, and all obviously torn up from the regular strata of the country by the same disturbing forces which formed the ancient gravel. Among these fragments, and among rounded blocks of stone chiefly derived from the same strata, are many minute fragments of bones, and sometimes entire teeth of various animals, more especially of the horse, the ox, the deer, and various graminivora. Among these, the remains of animals (such as the mammoth and the rhinoceros) now unknown as the inhabitants of any part of Europe are by no means uncommon. To describe, or even to enumerate, such specimens in detail would be foreign to my present purpose. I shall only refer, by way of example, to some of the organic spoils derived from the undisturbed *diluvium* in the neighbourhood of Cambridge. 1. Fragments of the pelvis of a mammoth, from the gravel south of St. Ives, Huntingdonshire. 2. Grinder of the mammoth, from the *diluvium* which stretches from St. Ives towards the centre of the fens. 3. Fragments of a large mammoth's tusk, from Foulmire. 4. A very large grinder of the mammoth, from the gravel

beds at Hinxton. 5. Innumerable fragments of the bones of various animals from the beds of small flint-gravel, north-west of Cambridge. 6. Eight or ten fragments of mammoth's grinders, from the thick gravel beds behind Barnwell. 7. Three or four large and perfect grinders of the mammoth, from the fine flint-gravel south of Cambridge; along with which were found several bones of the horse, and teeth of various graminivora. 8. Many teeth of various graminivora; humerus of a very large mammoth; several teeth of the rhinoceros; horns and portions of two enormous skulls of the urus or buffalo; an atlas (probably belonging to one of the preceding species), in linear dimensions about twice as large as the atlas of a full grown ox; several perfect bones of the horse; fragment of the horn of the *cervus giganteus*; &c. &c. all derived from the gravel beds at the north-west end of Barnwell.\*

Such are the organic remains contained in a small part of the *diluvium* of this country; all of them differing in condition, and many of them differing in kind from the corresponding spoils of the *alluvial* beds of the same district; and the distinction is rendered still more complete by the fact, that not one work of human art, and not one fragment of a human skeleton, have yet been discovered in any part of the numberless excavations which are conducted in the lower and more ancient deposit. When we properly estimate these facts (which are but the counterpart of some of the admirable details given in the "*Reliquæ Diluviana*"), and consider how very small a portion of the superficial gravel has yet been turned over even in the most populous parts of our island. We are compelled to admit that animals almost without number must have inhabited all the lower parts of Europe before the commencement of those destructive operations which produced the *diluvial* gravel.† A further examination of the facts already stated leads us also to conclude, that many pre-existing species of animals must have perished during the operation of the same destructive causes; because we do not find their remains in any more recent deposit.

It is in vain for any one to attack these conclusions by demanding how it came to pass that one class of animals perished during the formation of the diluvial gravel, and another class survived it. The same difficulty meets us in classing many of the regular strata of the earth. The suite of fossils derived from one formation may be widely different from the suite derived from another; yet we know by experience that both suites may contain many individuals of a common species.

\* Most of the specimens from this locality are in the possession of J. Okes, Esq. of Cambridge.

† This conclusion had been completely demonstrated, in the opinion of most geologists, from the number, the nature, and the condition of the organic remains of the gravel: had any doubt remained on the subject, it is now set at rest by the details connected with the Kirkdale cavern given in the "*Reliquæ Diluviana*."

Still less are the conclusions shaken by the hypothesis, that the weapons of the hunter completed the extinction of many species of animals, of whose former existence we have no knowledge, except through their bones, which are buried in the beds of old *diluvial* covering. From the only physical evidence which we can have on such a subject, we believe that not a single hunter had ever trodden in the woods of Europe at the time when the mammoth, the rhinoceros, and the hyæna were its inhabitants. And the records of Europe afford no proof that such beasts ever inhabited this part of the world in times within the reach of history. Again, we know by direct evidence, which is independent of any zoological details and of any history, that the diluvial gravel is of great antiquity; and we know from history that in ancient times large tracts of Europe existed in the form of unreclaimed marsh or almost impenetrable forest. Under such circumstances, are we to believe that a set of inhabitants, savage, almost naked, and few in number, should have waged a war of extermination with large and formidable beasts like the rhinoceros, the cavern-bear, and the hyæna? The hypothesis which attributes the extinction of such animals to the agency of hunters in the early ages of the world is at once gratuitous and incredible.

As the general result of all the preceding details, we may conclude that the separation of the superficial debris of the earth into two classes (*diluvial* and *post-diluvial detritus*), formed by different causes, and during distinct epochs, is completely made out; first, by the direct evidence of natural sections proving one formation superior to the other; secondly, by the distinct suites of organic remains imbedded in the two deposits. The lower formation containing many organic remains which are never found in the upper; and the upper also containing many which are not found in the lower. In these respects, perhaps, no two contiguous formations in the crust of the earth are separated from each other by more clear and decisive characters.

#### SECT. 5.—On the Causes of Diluvial and Alluvial Phenomena.

The conclusions which I have attempted to vindicate in the preceding sections, however interesting in themselves, give us but scanty means of speculating on the causes which have produced the *diluvial* deposits. It may be asked, by what forces were the diluvian torrents first put in motion? In what direction did they sweep over the earth? On what part of the earth's surface have they acted? Did they operate almost simultaneously over all parts of the world, or did they act at intervals and during a long period of time? What was the condition of the globe prior to their action, and what are the modifications in its external character produced by them? To some of these questions, no answer can be given, and to none of them can we give

a complete answer in the present state of our information. If, however, a great many well observed facts seem to point to one conclusion, that conclusion must be considered probable until it is opposed by some other conflicting facts. One thing at least is certain, that no hypothesis can be admitted which is not borne out by that series of facts (however imperfect) with which we are now acquainted.

On these grounds I do not hesitate a moment in rejecting the hypothesis which allows the formation of *alluvial* deposits in the manner above described, but accounts for all the *diluvial* phenomena by a succession of partial and transient inundations, occasioned by the bursting of lakes, and other similar catastrophes.\* In the first place, the cause assigned is inadequate to the effects produced. The physical contour and structure of the central and southern parts of England show the impossibility of any large lakes ever having existed among our secondary strata, capable of producing the enormous and almost continuous beds of gravel which stretch along the eastern coast. Several striking facts connected with this question have fallen under my own observation; and, as far as they go, confirm the general views given in the "Reliquiæ Diluviorum." As the description of these facts will lead me into some details, I hope to resume the subject in the next number of the *Annals of Philosophy*. Secondly, the hypothesis is gratuitous. In many parts of England, where there is abundance of superficial gravel, there is not the shadow of evidence to prove that any great body of water was ever pent up among the neighbouring strata, so as to form a lake which afterwards burst the barriers by which it was confined. Catastrophes of this kind sometimes happen in mountainous regions, and the effects produced are commensurate to the agents; but these effects have nothing to do with the great masses of superficial gravel even in the contiguous districts.† Thirdly, the feeble agents which the hypothesis allows would require an indefinite extension of time before they could produce such effects as the earth's surface plainly exhibits. But the quantity of marsh land and silt formed at the head of many lakes, the extent of different deltas, and other similar phenomena, appear to demonstrate that all *alluvial* deposits have been completed within a very limited period.‡ The hypothesis is, therefore, inadmissible, which makes *alluvial* and *diluvial* deposits contemporaneous, and implies an indefinite period of

\* This appears nearly to agree with Saussure's opinions, and is still held by some geologists on the Continent.

† In consequence of the prevalence of local disturbing forces, such as those alluded to in the text, the great relations of the superficial *détritus* cannot be studied to so much advantage in the immediate neighbourhood of mountain chains, as in the lower regions of the earth's surface.

‡ We owe this conclusion to Deluc who devoted the labours of many years to its confirmation. Had his labours terminated here, he had done great service to geology.

time for their formation. Fourthly, the hypothesis does not account for the different suites of organic remains found in each deposit. Lastly, it does not account for the constant order in the position of *alluvial* and *diluvial* debris. Had they been formed in the way which the hypothesis implies, they must sometimes have alternated. Each of these objections might be expanded and illustrated by many details; but to enter on them would be foreign to my present purpose.

The details already given in the preceding sections sufficiently explain the origin of common *alluvial* formations. But there are two classes of phenomena exhibited on several parts of the coast of our island, which are intimately connected with the present inquiries, and do not always admit of easy explanation, viz. 1. Traces of recent marine deposits above the level of high-water. 2. Extensive traces of ancient forests in situations which are constantly overflowed at high-water.

Phenomena of the first class are generally met with on the banks of estuaries where the waters of the sea necessarily undergo great oscillations. By the extraordinary combination of a high spring tide, and a hurricane blowing in the direction of the current, whales and other marine animals have from time to time been stranded on the banks of estuaries in situations 20 or 30 feet above the reach of common floods.\* This is not mere hypothesis: we know that by the combination of such circumstances as these, the sea has two or three times, within the last 600 years, risen to an extraordinary elevation on the coast of Holland, and overwhelmed large and populous tracts of that country.

The existence of *submarine* forests is not so readily accounted for. Some writers have supposed them to be the effects of earthquakes, which in ancient times have submerged large tracts of forest land bordering on the sea coast. Without pretending to exclude such agents in cases which without them admit of no explanation, I think that in a vast majority of instances it is unnecessary to introduce them. The mean elevation of the sea about every part of our coast is unquestionably constant; but the actual level of high-water at any given place is dependent on the velocity and direction of the tidal currents, the contour of the coast, and a number of circumstances which are entirely local. In proof of this assertion, it is only necessary to appeal to the fact, that in extensive bays and estuaries, the sides of which gradually diverge towards the open sea, the tides occasionally rise (through the operation of a common hydrostatical

\* Two examples of this kind are noticed by Dr. Fleming in the last number of the Edinburgh Philosophical Journal, p. 124. Such cases must be carefully distinguished from all tertiary deposits; and from such accumulations of marine shells as are seen in the *crag-pits* on various parts of Norfolk, Suffolk, and Essex. These latter instances unquestionably belong to no natural marine inundation, and are, at least, as old as the *diluvium* in that part of England.

law) to an elevation which is many times greater than the rise of the same tides on more open parts of the coast. Any set of causes which greatly modify the form of a deeply indented coast, must, therefore, inevitably produce considerable local effects upon the level of high-water.

Let these remarks be applied to the eastern shores of England. We know that during the last 1000 years, the sea has made enormous encroachments on many parts of Suffolk, Norfolk, Lincolnshire, and Yorkshire; not only modifying the whole contour of the coast, but at the same time forming chains of shoals and sandbanks by which the velocity and the direction of the tidal currents must have been more or less affected. The waters have, therefore, during successive ages, been propelled into the recesses of the coast by different forces, and up different systems of inclined planes; and must in consequence have ascended to different levels. Such effects as these will reach their maximum on the shores of large bays and estuaries, like the Humber and the Wash of Lincolnshire.

The form of the Wash of Lincolnshire must have been greatly changed since the epoch of the *diluvial detritus*, partly by the degradation\* of the neighbouring cliffs: but still more by the encroachments of *alluvial* silt which has been pushed down into it by the waters of the Witham, the Glen, the Welland, the Nene, and the Ouse.\* If an undulating line be drawn through these several rivers a few miles above the estuaries in which they terminate, it may be taken as an approximation to the form of a part of the coast in very ancient times before the great accumulation of *alluvial* matter. The country within this line then presented a low undulating surface, gradually rising on every side of the Wash towards the high lands; and it was probably almost covered with forest trees, with the exception of a few very low regions through which the rivers descended to the sea, and which were partially flooded at the time of high-water. But in the present state of things, the flood-tides, after filling the lower part of the Wash, are pushed on towards the ancient line of coast through a number of estuaries, the sides of which converge towards the interior, and on that account force the waters up to a higher level than they could reach on a coast which was less indented. And after the flood-tides have been thus pushed up into the mouths of the rivers, they do not now, as in former times, mix with the freshwater and cause a reflux, extending far into the interior of the country; but after rising, almost at once, to a high level,† they are pent up between artificial banks, and soon stopped altogether by *locks* and other works connected

\* A long note containing some details connected with the drainage of the fens bordering on the Wash, arrived too late for the press, but will be affixed to the continuation of this paper.—Edit.

† See note 7, p. 244.

with the artificial drainage and navigation of the country. It is almost certain that in such a state of things the tides cannot rise to the exact level which they reached in ancient times; and the change will, I think, be precisely of that kind which will explain the appearance of submarine forests in many places bordering upon the Wash. If through a combination of causes such as have been mentioned, the tides on any part of the coast rise to a level only a few feet higher than they did in ancient times, the whole difficulty we have been considering at once vanishes.

The conclusions which have been deduced from a consideration of certain facts exhibited on the coast of Lincolnshire, may be extended to every country which is similarly circumstanced; and it seems probable that an actual change in the height of the tides produced by a change in the contour of the neighbouring coasts, is among the most general and efficient causes which have produced the phenomena of submarine forests. By this assertion it is, of course, never intended to exclude other agents from their proper share in producing the phenomenon. Forest trees may have grown in many low tracts bordering on the sea while they have been protected from the flood-tides by artificial, and sometimes, perhaps, by natural embankments; and in subsequent ages the embankments may have failed, and the forests may have been submerged by a consequent incursion of the waters. Fen lands, after being drained and brought under cultivation, may have undergone a natural subsidence, and on that account have been exposed to the chance of subsequent inundations. This at least was Deluc's opinion, founded on observations made in various parts of Holland. Lastly, large tracts of low alluvial land may (after the natural destruction of the barriers by which they were held in) be transferred by a slide to a lower level; and in that way productions once out of the reach of the high tides may become exposed to their constant attacks. By the gradual operation of such causes as have been enumerated, the existence of submarine forests may in most instances be satisfactorily explained without the intervention of earthquakes or other irregular disturbing forces.

The phenomena above described (viz. the existence of land productions below, and of marine productions above the level of high-water) are after all things *sui generis*, which are confined to a small part of the coast; and, however interesting in themselves, throw no light whatever on the general classification of alluvial and diluvial deposits.

(To be continued.)

## ARTICLE II.

Corrections in Right Ascension of 37 Stars of the Greenwich Catalogue. By James South, FRS.

	Pegasi	Polaris	$\alpha$ Arietis	$\alpha$ Ceti	Aldebaran	Capella	Rigel	$\beta$ Tauri	$\alpha$ Orionis
Mean AR 1825.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
	0 4 14.2	0 58 17.5	1 57 19.77	2 53 8.58	4 25 53.41	5 3 45.61	5 6 8.00	5 15 14.29	5 45 42.18
April 1	+ 0.60"	- 32.98"	+ 0.73"	+ 0.80"	+ 1.30"	+ 1.87"	+ 1.17"	+ 1.70"	+ 1.59"
2	62	32.95	73	80	29	85	15	68	57
3	63	32.92	73	79	28	83	14	67	56
4	64	32.89	73	79	26	82	12	65	54
5	66	32.86	73	78	25	80	11	64	53
6	67	32.84	73	78	24	78	09	62	51
7	68	32.81	74	77	23	76	08	61	50
8	69	32.78	74	77	22	74	06	59	48
9	70	32.75	74	76	20	72	05	57	46
10	72	32.72	74	76	19	70	03	56	45
11	73	32.59	75	76	18	68	02	55	44
12	75	32.46	75	76	17	67	01	53	42
13	76	32.33	75	76	16	65	0.99	52	41
14	78	32.20	76	76	15	64	98	51	40
15	79	32.06	76	76	14	62	97	49	39
16	81	31.84	77	75	13	61	96	48	37
17	82	31.63	77	75	13	59	95	47	36
18	84	31.41	78	75	12	58	94	46	35
19	85	31.19	78	75	11	56	92	45	33
20	87	30.97	79	75	10	55	91	44	32
21	89	30.71	80	75	09	54	90	43	31
22	91	30.45	81	76	09	52	89	42	30
23	93	30.18	82	76	08	51	89	41	30
24	95	29.91	83	76	08	50	88	40	29
25	97	29.63	84	77	07	49	87	39	28
26	99	29.29	85	77	07	48	86	39	27
27	1.01	28.95	86	77	07	47	85	38	26
28	03	28.61	87	78	06	46	85	37	25
29	05	28.27	88	78	06	45	84	36	24
30	07	27.92	90	78	06	44	83	35	24
May 1	09	27.54	91	79	06	43	83	35	23
2	12	27.16	92	79	06	43	82	34	22
3	14	26.78	94	80	06	42	82	34	21
4	16	26.39	95	81	06	42	82	33	20
5	18	26.00	96	81	06	41	81	33	19
6	21	25.54	97	82	06	41	81	33	18
7	23	25.08	99	83	06	40	81	32	17
8	25	24.61	1.00	84	06	40	80	32	16
9	28	24.14	01	85	06	39	80	32	15
10	30	23.67	03	86	06	39	79	31	15
11	33	23.18	05	87	07	39	79	31	15
12	35	22.69	07	88	07	39	79	31	15
13	38	22.19	09	90	07	39	79	31	14
14	40	21.69	11	91	08	39	79	31	14
15	43	21.18	13	92	08	39	79	31	14
16	46	20.65	15	93	09	39	78	31	14
17	48	20.12	17	94	09	39	78	31	14
18	51	19.58	19	96	10	39	78	31	14
19	53	19.04	21	97	10	39	78	31	13
20	56	18.50	23	99	11	39	78	31	13
21	59	17.92	25	1.01	12	40	78	31	13
22	62	17.34	28	02	12	40	79	32	13
23	64	16.76	30	04	13	41	79	32	13
24	67	16.17	33	06	14	41	80	32	13
25	70	15.58	35	07	15	42	80	33	13
26	73	14.94	37	08	16	43	81	33	14
27	76	14.39	40	10	17	43	81	33	14
28	79	13.74	42	11	18	44	81	34	14
29	82	13.08	44	13	19	45	82	34	14
30	85	12.32	47	16	20	46	82	35	14
31	88	11.65	49	18	21	47	83	36	15

Mean AR 1825.	Sirius		Castor		Procyon		Pollux		$\alpha$ Hydræ		Regulus		$\beta$ Leonis		$\beta$ Virginis		Spica Virg.	
	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
	6 37	26 11 7	23 25 30	7 30	8 47 7	34 35 8	9 18	39 40 9	59 2 78	11 40 7 90	11 41 34 98	13 15 59 22						
April	1	+ 1° 51"	+ 2° 48"	+ 2° 10"	+ 2° 46"	+ 2° 50"	+ 2° 83"	+ 3° 02"	+ 3° 01"	+ 3° 11"								
	2	49	46	09	44	49	82	02	02	12								
	3	47	44	07	42	47	81	02	02	13								
	4	46	45	05	41	46	80	01	03	14								
	5	44	41	04	39	45	79	01	03	14								
	6	42	40	03	38	44	78	01	04	15								
	7	40	38	01	36	42	77	01	04	16								
	8	39	36	1 99	34	41	76	01	05	17								
	9	37	34	97	32	39	75	00	05	18								
	10	35	32	95	30	38	74	00	06	19								
	11	33	30	93	28	37	73	00	05	19								
	12	32	28	92	27	35	72	2 99	05	20								
	13	30	27	90	25	34	71	99	04	20								
	14	28	25	89	24	33	70	98	03	21								
	15	26	23	87	22	32	69	98	02	21								
	16	24	22	86	20	30	68	98	01	22								
	17	23	20	84	19	29	67	97	01	22								
	18	21	19	83	17	28	66	97	00	23								
	19	19	17	81	16	26	65	96	2 99	23								
	20	18	15	79	14	25	63	96	98	24								
	21	17	14	78	12	24	62	95	98	24								
	22	15	12	76	11	22	60	95	97	24								
	23	14	10	75	09	21	59	94	97	24								
	24	13	09	73	08	19	58	94	96	25								
	25	11	07	72	06	18	57	93	96	25								
	26	10	06	71	05	16	55	93	95	25								
	27	09	04	69	03	15	54	92	95	25								
	28	07	03	68	01	14	53	92	94	25								
	29	06	01	66	00	12	51	91	94	26								
	30	04	1 99	65	1 98	11	50	90	93	26								
May	1	03	98	64	97	10	49	89	92	26								
	2	02	96	62	95	08	48	89	92	26								
	3	01	95	61	94	07	46	88	91	26								
	4	0 99	94	60	93	05	45	87	90	26								
	5	98	92	58	92	04	44	86	89	26								
	6	97	91	57	90	03	43	85	88	26								
	7	96	90	56	89	01	42	85	87	26								
	8	94	88	55	88	00	40	84	86	26								
	9	93	87	54	86	1 98	39	83	86	26								
	10	92	86	53	85	97	38	82	85	26								
	11	91	84	52	84	96	37	81	84	26								
	12	90	83	51	83	95	36	80	83	26								
	13	89	82	50	81	93	34	79	83	25								
	14	88	81	49	80	92	33	78	82	25								
	15	87	79	48	79	91	32	77	81	25								
	16	86	78	47	78	90	31	76	80	25								
	17	85	77	46	77	89	30	75	79	25								
	18	84	76	45	76	87	28	74	79	24								
	9	84	75	44	75	86	27	73	78	24								
	20	83	74	44	74	85	26	72	77	24								
	21	83	73	43	73	84	25	71	76	24								
	22	82	72	42	72	83	24	70	75	23								
	23	82	72	42	71	81	23	69	74	23								
	24	81	71	41	71	80	22	68	73	22								
	25	81	70	40	70	79	21	67	72	22								
	26	80	69	40	69	77	20	66	71	22								
	27	80	69	39	68	76	18	65	70	21								
	28	79	69	38	68	75	17	64	69	21								
	29	79	68	38	67	74	16	63	68	20								
	30	78	68	37	66	73	15	62	67	19								
	31	78	68	37	66	72	14	61	66	19								

Arcturus				2 <sup>a</sup> Libræ				Cor. Bor.				Serpent.				Antares				Herculis				Ophiuchi				Lyra				A. juv.			
Mean AR.	h.	m.	s.	h.	m.	s.	h.	m.	s.	h.	m.	s.	h.	m.	s.	h.	m.	s.	h.	m.	s.	h.	m.	s.	h.	m.	s.	h.	m.	s.	h.	m.	s.		
1925.	14	7	41.06	14	41	12.92	15	27	16.97	15	35	39.42	16	18	41.57	17	6	40.44	17	26	49	02	18	31	07.01	19	37	56.5							
April	1	+	2.86"	+	3.11"	+	2.55"	+	2.67"	+	3.04"	+	2.18"	+	2.11"	+	1.35"	+	1.45"																
	2		87		13		57		69		07		23		14		38		18																
	3		89		14		59		71		09		23		17		42		51																
	4		90		16		61		73		12		26		20		45		54																
	5		91		17		63		75		15		28		22		48		56																
	6		92		19		65		78		18		31		25		52		59																
	7		93		20		67		80		21		33		27		55		60																
	8		95		22		69		82		24		36		30		58		63																
	9		96		24		72		84		26		39		32		62		68																
	10		98		26		74		86		28		41		35		65		71																
	11		99		27		76		88		31		43		37		69		74																
	12	3.00			29		77		89		33		46		40		72		77																
	13		01		30		79		91		36		48		42		75		79																
	14		02		31		80		93		38		51		45		79		82																
	15		03		33		82		95		41		53		47		82		87																
	16		04		34		84		96		44		56		50		85		88																
	17		04		36		86		98		46		58		52		88		91																
	18		05		37		87		3.00		49		61		55		91		93																
	19		06		39		89		01		52		63		57		94		96																
	20		07		40		91		03		54		66		60		97		99																
	21		08		41		92		05		56		68		62		2.00		2.02																
	22		08		43		94		06		58		70		64		03		04																
	23		09		44		95		08		61		73		67		06		07																
	24		10		45		97		09		63		75		69		09		10																
	25		10		46		98		11		65		77		71		12		14																
	26		11		47		99		13		68		79		73		15		17																
	27		11		48		3.01		15		70		82		75		18		20																
	28		12		49		02		16		72		84		77		21		22																
	29		12		50		04		18		74		86		79		24		25																
	30		13		51		05		19		76		88		81		27		28																
May	1		13		52		06		20		78		90		83		30		31																
	2		13		53		07		21		80		92		86		33		34																
	3		13		54		08		23		82		94		88		36		37																
	4		13		54		09		24		84		96		90		39		40																
	5		13		55		10		25		86		98		93		42		43																
	6		14		56		11		26		88		3.00		95		44		45																
	7		14		56		12		27		89		02		98		47		48																
	8		14		57		13		29		91		04		3.00		50		51																
	9		14		58		14		30		93		06		03		53		54																
	10		15		59		15		31		95		08		05		56		57																
	11		15		60		16		32		97		10		07		59		60																
	12		15		60		17		34		98		12		09		61		62																
	13		15		61		17		34		3.00		13		10		64		65																
	14		15		61		18		35		01		15		12		66		68																
	15		15		62		19		36		03		16		14		69		70																
	16		16		63		20		37		05		18		16		71		73																
	17		16		64		21		38		06		19		18		74		76																
	18		16		64		21		38		08		21		19		76		78																
	19		16		65		22		39		09		23		21		79		81																
	20		16		65		23		40		11		24		23		81		84																
	21		16		65		23		41		12		26		25		83		87																
	22		15		65		24		41		13		27		26		85		89																
	23		15		65		24		42		15		28		28		87		92																
	24		15		66		25		43		16		30		29		89		95																
	25		14		66		25		44		17		31		31		92		97																
	26		14		66		26		44		19		33		33		94		3.00																
	27		14		66		26		45		20		34		34		96		03																
	28		13		66		26		45		21		36		36		98		05																
	29		13		67		27		46		23		37		37		3.00		08																
	30		13		67		27		46		24		38		39		03		10																
	31		13		67		27		46		25		39		40		05		12																



	γ Pegasi		Polaris		α Arietis		α Ceti		Aldebaran		Capella		Rigel		β Tauri		α Orionis	
Mean AR 1825.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	
	1 0 4 14	25 0 58	17 50 1 57	19 77 2 53	8 58 4 25	53 44 5 3 46	61 5 6 8 40	5 15 14	29 3 45	42 15								
June	1	1-91"	-10-98"		+ 1-52"	+ 1 20"		+ 1-23"		+ 1-49"		+ 0-84"		+ 1-37"		+ 1-15"		
2	94	10-32	54	22	24	50	84	36	15									
3	97	9-65	57	24	25	51	85	39	16									
4	2-00	8-09	60	26	27	52	86	40	16									
5	03	8-28	62	28	28	53	87	41	17									
6	06	7-57	65	30	29	54	87	42	17									
7	09	6-85	67	32	31	55	88	43	18									
8	12	6-13	70	34	32	57	90	44	18									
9	15	5-40	73	37	34	59	90	45	19									
10	18	4-68	76	39	36	61	91	46	20									
11	21	3-95	79	42	38	62	92	48	21									
12	24	3-22	82	44	39	64	94	49	22									
13	28	2-49	85	46	41	66	95	51	23									
14	31	1-76	88	49	43	67	96	52	24									
15	34	1-00	91	51	45	69	98	54	25									
16	37	0-24	94	54	46	71	99	55	26									
17	40	0-52	97	56	48	73	1-00	57	27									
18	44	1-28	2-00	59	50	75	02	58	28									
19	47	2-03	03	61	52	77	03	60	30									
20	50	2-79	06	64	54	79	05	62	31									
21	53	3-55	09	66	56	81	06	63	33									
22	57	4-32	12	69	58	84	08	65	34									
23	60	5-09	15	71	60	86	09	67	35									
24	63	5-85	18	74	63	88	11	69	37									
25	66	6-65	21	76	65	91	12	71	38									
26	69	7-46	24	79	67	93	14	73	39									
27	72	8-27	28	81	69	95	16	75	41									
28	75	9-09	31	84	72	98	17	77	42									
29	78	9-90	34	87	74	2-00	19	79	43									
30	81	10-70	37	90	76	03	21	81	45									

	Sirius		Cantor		Procyon		Pollux		α Hydre		Regulus		Leonis		β Virginis		Spica	
Mean AR	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.	h. m. s.
1825.	1 6 37	26 11 7	23 23 30	7 30 8	17 7 34	26 55 9	18 50 10	59 2 25	11 40 28	11 41 34	9 13 12							
June 1	1	+ 0-78	+ 1-67		+ 1-37	+ 1-65		+ 1 71		- 2-13		- 2-60		- 2-65		- 3-15		
2	78	67	36	65	71	12	59	64	15									
3	78	67	36	65	70	11	58	63	17									
4	78	66	36	65	69	10	57	62	16									
5	78	66	35	64	68	09	56	62	16									
6	77	66	35	64	67	07	55	61	15									
7	77	66	35	64	66	06	54	60	14									
8	77	65	35	64	66	05	53	59	14									
9	77	65	34	63	65	04	52	58	13									
10	77	65	34	63	64	03	51	57	12									
11	78	65	34	63	64	02	50	56	12									
12	78	65	34	63	63	02	48	55	11									
13	78	65	34	63	62	01	47	54	10									
14	79	65	34	63	61	00	46	53	09									
15	79	66	35	63	61	1-00	45	52	09									
16	80	66	35	63	60	99	44	51	08									
17	80	66	35	63	59	98	42	50	08									
18	81	66	35	63	59	97	41	49	07									
19	81	66	35	63	58	96	40	48	06									
20	82	66	35	64	58	95	39	47	05									
21	82	67	36	64	57	95	38	46	04									
22	83	67	36	64	57	94	37	45	03									
23	84	67	36	65	56	94	36	44	02									
24	84	68	37	65	56	93	35	43	02									
25	85	68	37	65	55	92	34	42	01									
26	86	69	38	66	55	92	33	41	00									
27	86	69	38	66	54	91	32	40	2-00									
28	87	70	39	67	54	91	31	39	98									
29	88	71	39	67	53	90	30	38	97									
30	89	72	40	68	53	90	29	37	96									



## ARTICLE III.

Results of a Meteorological Register kept at Helston, Cornwall, in 1824. By Mr. M. P. Moyle.

1824.	BAROM. *	THERMOMETERS.			30 feet high: in 1000ths.	PNEUMOMETER.		WIND.	WEATHER.		
		Self-registering. In the shade.	Monthly mean.	Self-registering. In the sun.		Monthly mean.	(Common) month- ly mean.		5 feet.	Mean.	Prevailing wind.
January	29.5804	43.33		46.55	40.33	1.10	1.67	1.535	SW	14	17
February	29.7767	44.62		45.50	44.02	2.53	2.89	2.710	SW	20	9
March	29.9034	43.64		49.00	41.01	3.86	4.34	4.120	SW	13	18
April	29.8678	47.60		52.75	47.09	2.61	2.69	2.650	E	17	13
May	29.9576	52.30		58.24	53.71	3.46	3.23	3.345	E	10	21
June	29.9692	58.25		63.65	58.64	4.80	4.89	4.845	E	12	18
July	29.90175	61.54		69.59	61.40	3.05	3.23	3.140	SW	11	20
August	29.9506	60.61		65.50	62.28	1.98	2.26	2.120	W	11	20
September	29.8692	55.33		67.00	57.00	3.65	3.83	3.740	W	16	14
October	29.8544	55.43		60.80	55.00	4.70	4.78	4.740	W	22	9
November	29.7141	49.50		52.00	49.86	5.02	5.33	5.175	SW	21	9
December	29.9312	47.02		49.62	47.76	4.28	4.25	4.280	SW	24	7
Annual means, &c.	29.8736	51.600	57.083	51.758	11.84	13.46		42.400	SW	191	175

\* 105 feet above sea level; corrected to 32° of Fahr.

## BAROMETER.

Highest, Jan. 16. Wind E. .... 30.6692  
 Lowest, Oct. 11. Wind var. from W to S. 28.4089

## THERMOMETERS.

*Registering, in the Shade.*

Highest, July 23. Wind SW. .... 73°  
 Lowest, Jan. 16. Wind E. .... 29

*Registering, in the Sun.*

Highest, Aug. 26. Wind E. .... 100°  
 Lowest, Jan. 16. Wind E. .... 29

*Common, in the Shade.*

Highest, July 23. Wind SW. .... 73°  
 Lowest, Jan. 5. Wind E. .... 31

Wet days comprehend rainy, showery, foggy, snowy, and those in which there was a fall of hail.

One of the pluviometers is situated on the top of a chimney thirty feet from the ground, the other five feet only; both are *apparently* free from the operation of local causes; but the lowest has been found, with scarcely an exception, to exceed considerably the other, and in the whole amounts to more than two inches in the year.

The journal consists of three observations daily, viz. from 8 to 9 a.m.; 3 p.m.; and from 10 to 11 p.m.; and from the means of those periods the barometer appears to be highest at the night observation, next high at the morning observation, and lowest at three, or the noon observations. The means of the three daily observations are always recorded.

*January.*—A very fine month. A few gentle hailshowers, but no snow, and scarcely ice enough to continue 12 hours.

*February.*—Many days wet, but only two on which there was a little frost and snow.

*March.*—A very wet month. A heavy storm on the night of the 9th, and morning of the 10th, of hail, rain, &c. Colder than February.

*April.*—A boisterous and wet month, with principally an easterly wind.

*May.*—A fine month with much easterly wind.

*June.*—Very wet, stormy, and cold. On the 9th there was much thunder, lightning, &c. with a variable wind from E to S and W.

*July.*—This was also a wet month. On the 14th there was a severe thunder storm, and the tide on the sea coast was observed to recede suddenly below low water-mark, and instantly return with great velocity to full tide.

*August.*—Thunder and lightning on the day and evening of the 2d, accompanied with much rain.

*September.*—Many days of heavy rain.

*October.*—Remarkably wet, having 22 days rain. The barometer fell very low on Sunday the 10th and Monday the 11th, accompanied by a heavy gale of wind, with thunder and lightning.

*November.*—A very wet and gloomy month. A violent hurricane on the night of the 26th and morning of the 27th; wind S and SE. Rivers had overflowed their banks; the tide rose to an unprecedented height, and much damage was done along the southern coast.

*December.*—There were 24 wet days in this month, and the remaining seven were so damp and disagreeable as to amount almost to such. There were but two decidedly *dry days* for the month. On the 4th, there was a heavy thunder storm in the evening. A fine meteor was observed on the night of the 29th, about nine o'clock.

Altogether the mean temperature of the year is much below the usual standard.

If there be added to the mean height of the barometer the sum of 0.121 in. for its elevation above the sea level, which is very near the truth, it will give for the mean height  $29.8736 + 0.121 = 29.9946$ .

It appears remarkable that there was not a calm day observed for the year; a few hours only of calm occurred at a time. In this observation, I consider it necessary that an extensive sheet of water should not be rippled; leaves of trees and long grass should not be perceived to move, and the smoke ought to ascend perpendicularly, to indicate a calm. This I believe to be a very rare circumstance for many hours in succession.

#### ARTICLE IV.

*Some Experiments with Oxide of Uranium and its Combinations.*  
By Jac. Berzelius.\*

THE Transactions of the Royal Academy of Sciences for 1822, contain a copious memoir on uranium by M. Arfwedson, which has extended and at the same time considerably altered our former ideas respecting that metal.† Among the experiments which he made with a view to determine the composition of the oxide, one gave 5.56 parts of oxygen to 100 parts of uranium, and two others 6.24 and 6.37; on the contrary he found, without any variation in his results, that in the oxidule, 100 parts of

\* Kong. Vet. Acad. Handl. 1823, St. 1.

† For a translation of M. Arfwedson's memoir, see *Annals*, vii. 253, New Series.

uranium are combined with 3.688 parts of oxygen. According to the former of the above results, which M. Arfwedson considers as the most accurate, the ratio of the oxygen in the oxide and oxidule is as 3 : 2; according to the latter, it is as 5 : 3. As the oxide of uranium readily acts as a weak acid, it appeared to me not unlikely that the latter ratio might be the true one, and I thought it of importance to determine the point with precision. I undertook, therefore, an investigation of the composition of the oxide of uranium, in order by that means to ascertain its saturating capacity as an acid; \* but the experimental determination was accompanied with so many difficulties, that I began to doubt its possibility. The oxide of uranium or its hydrate cannot be obtained artificially in a state of purity. If we attempt its preparation by means of nitric acid, it passes into the state of oxidule just at the instant when we expel the last portions of acid. If we precipitate it with an alkali, it combines with the precipitant; and when the latter is of a fixed nature, the compound may be ignited without undergoing decomposition. Owing to the presence of ammonia in the hydrate precipitated by that alkali, it is impossible to analyse it with such precision, that the proportion of oxygen will be determined to within less than  $\frac{1}{100}$  of the weight of the oxide.

I next hoped to gain my object by analyzing the carbonated oxide. I therefore precipitated a solution of the nitrate of oxide of uranium with carbonate of ammonia. No effervescence took place at the commencement of the precipitation, and the precipitate, on being collected upon a filter, appeared at first to admit easily of a complete edulcoration; but it speedily became whiter coloured, and at the same time so much of it passed into solution, that it imparted a distinct yellow colour to the filtered liquid. This liquid became turbid on the application of heat, and acquired a light yellowish milky appearance, but many days elapsed before the whole of the oxide of uranium subsided to the bottom. The residue upon the filter dissolved in acids without effervescence, and was therefore hydrate, instead of carbonate of uranium. Thinking that this would prove peculiarly serviceable for my purpose, I ignited a quantity in a suitable apparatus, in which the expelled gas was collected over mercury, and the water by muriate of lime. By this means I determined with precision the weight of the oxidule and of the water; but the gas greatly exceeded the quantity of oxygen which ought to have been evolved, and proved to contain a considerable proportion of carbonic acid and azote. Consequently this hydrate was contaminated both with carbonic acid and with ammonia: it is probable that these two substances

\* Before commencing this investigation, I examined the oxidule in the same manner as had been done by M. Arfwedson, by reducing it with hydrogen gas, and found it composed of 100 uranium + 3.685 oxygen.

were associated together, and were retained by the hydrate in the state of carbonate of ammonia.

I now examined the oxalate of the oxide. By submitting this salt to distillation, I decomposed it in one experiment into metallic uranium, carbonic acid, and water; and on the supposition that one-fourth of the oxygen of the carbonic acid had been previously combined with the metal, it would have followed that the oxide is composed of 100 uranium + 6.14 oxygen. But in another experiment, I obtained a residue of oxide of uranium, and totally different proportions of carbonic acid and water. In both cases the oxalate had been prepared with an oxide purified in the manner recommended by M. Arfwedson, and precipitated by a long continued ebullition from its solution in carbonate of ammonia. I shall return to the consideration of this salt. I now ignited a mixture of determinate quantities of the oxidule and of nitrate of lead, with the expectation of forming an uranate of lead; but by this treatment, only a small portion of the oxidule combined with an additional dose of oxygen. I mixed the two substances together therefore in the state of solution, evaporated the mixture to dryness, and calcined the residue; but during the evaporation, the nitrate of lead crystallized in the first place, and the salt of the oxidule concreted into a mass over it; and during the ignition, the latter salt first underwent decomposition, and the unequal mixture of oxidule of uranium and nitrate of lead which remained, afforded the same unsatisfactory result as in the first experiment. I now dissolved a determinate quantity of magnesia in nitric acid, expecting, with its assistance, to precipitate a solution of nitrate of the oxidule, by means of an excess of caustic ammonia; but both in this experiment, and when I mixed a determinate quantity of magnesia with a solution of a determinate quantity of the oxidule in nitric acid, evaporating the mixture to dryness and calcining the residue, the results which I obtained were equally varying and undecisive.

I next had recourse to more indirect methods. M. Arfwedson had found that oxide of uranium gives with sulphuric acid and potash a double salt, in which its oxygen is to that of the potash as 3 : 2. I determined therefore to examine this salt, and was the more induced to do so by the uncommonness of this ratio between the oxygen of the two bases. I mixed a saturated solution of the oxide with a smaller quantity of sulphate of potash than was necessary to form with it the double salt, and committed the liquid to spontaneous evaporation. I considered it not improbable that if the oxide of uranium contains 3 atoms of oxygen, it might, like alumina and the oxides of iron and manganese, form a salt crystallizing in a similar manner with alum, which would have afforded a decisive proof of its atomic constitution. But no such salt could be obtained, and the

double salt merely formed an adhering crust of small crystals, which had no relation with the octahedron. 1·2 grammes of this salt, heated until it began to undergo fusion, gave off 0·042 grm. of pure water. The residue afforded a turbid solution in water, in consequence of its being partially decomposed into a sub and a super salt, but the liquid was rendered transparent by the addition of a few drops of muriatic acid. The oxide of uranium was precipitated with ammonia, and collected upon a filter; and as it is soluble in pure water, it was washed with a weak solution of sal ammoniac. Ignited, it weighed 0·623 grm. and had acquired a green colour. The filtered liquid was evaporated to dryness, and the residue was calcined, in order to expel the ammoniacal salts. The sulphate of potash which remained weighed 0·3515 grm. Admitting that the oxygen of the oxide is to that of the oxidule as 3 : 2, and that the deficiency in the analysis consisted of sulphuric acid, the composition of the salt, according to this experiment, would be .

		Containing oxygen.	Per cent.
Potash . . . . .	19·00	3·23	15·833
Oxide of uranium . . .	63·40	3·31	58·833
Sulphuric acid . . . . .	33·40	20·04	27·834
Water . . . . .	4·20	3·73	3·500
	120·00		100·000

Another portion of this double salt, prepared from a solution containing an excess of acid, appeared to possess exactly the same crystalline form with the preceding, but its yellow colour was considerably paler. On being analysed by the same process with the one employed above, with this exception that after the separation of the oxide of uranium, the sulphuric acid was precipitated by muriate of barytes, it yielded 6·5 per cent. of water, 50 per cent. of oxidule of uranium, 82 per cent. of sulphate of barytes, and 27 per cent. of sulphate of potash. This is equivalent to

		Containing oxygen.
Potash . . . . .	14·60	2·48
Oxide of uranium . . . .	50·84	2·53 (2·65!)
Sulphuric acid . . . . .	28·20	16·92
Water . . . . .	6·50	5·78

This experiment demonstrates that the quantity of oxygen is the same in both bases, and that the salt was mixed with a portion of an acid salt, which differed from it also in containing a larger proportion of water of crystallization.

M. Arfwedson found the ratio between the oxygen of the oxide of uranium and of the potash to be nearly as 3 : 2. Hence it would appear that his salt contained a portion of sulphate of

uranium, the more especially as he found alcohol capable of extracting sulphate of uranium from it, which was not the case with the salt which I analyzed. The aqueous solution of any salt gave a yellow precipitate with alcohol, but the supernatant liquid was colourless.

I now prepared the double muriate of oxide of uranium and potash, which may be obtained crystallized by slowly evaporating a liquid containing an excess of the muriate of oxide of uranium. The crystals are sometimes four-sided prisms with obliquely truncated extremities, and sometimes four-sided rhomboidal tables. I intended at first to have analyzed it by reduction in hydrogen gas, but I found that the water of crystallization cannot be expelled, without carrying along with it a portion of the acid, after which the salt is no longer completely soluble in water. It was necessary, therefore, to perform the analysis in the humid way. 1.5 grm. of the crystals, previously dried in the state of powder in a temperature of  $130^{\circ}$ , dissolved in water without leaving any residue. The solution, precipitated with nitrate of silver, gave 1.61 grm. of fused muriate of silver. The excess of silver was separated by muriatic acid; the oxide of uranium was then precipitated by ammonia, and washed with a solution of sal ammoniac. It was converted by ignition into 0.82 grm. of oxidule. From the remaining liquid, after the dissipation of the ammoniacal salts, there was obtained 0.412 grm. of muriate of potash, = 0.2606 grm. of potash.

If the deficit be regarded as water of crystallization, it will follow from this analysis, that the salt is composed of

Potash .....	26.06	containing oxygen	4.43
Oxide of uranium .	83.46		4.47
Muriatic acid ....	30.75	saturating capacity	9.05
Water .....	9.73		8.93

	By experiment.	By calculation.
Potash . . . . .	17.37	17.32
Oxide of uranium. .	55.64	55.98
Muriatic acid . . . .	20.50	20.11
Water . . . . .	6.49	6.59
	<hr/> 100.00	<hr/> 100.00

I have stated my results in conformity with the older theory respecting the constitution of muriatic acid, that I might be able to employ that acid as a standard. It is obvious that both the bases contain equal quantities of oxygen, and that the saturating capacity of the muriatic acid is exactly equivalent to the oxygen of the two bases. The differences between the experimental and calculated results are trifling, and may be safely imputed to the unavoidable errors of observation. I consider the result of

this analysis as more decisive than that of any of the preceding; for if the oxide of uranium were otherwise constituted, a very different quantity of muriate of silver ought to have been obtained. If, for example, we suppose that the uranium in this experiment had been combined with 4.96 instead of 4.47 parts of oxygen, the quantity of muriate of silver ought to have been 1.684 grm.; but the difference between this and the experimental result greatly exceeds what could possibly be occasioned by any errors of observation. Indeed if the oxide of uranium contained 5 atoms of oxygen, it is not probable that it could have existed associated with potash in the above-mentioned relation, where the oxygen of the oxidule is two-thirds that of the potash, but that the oxygen both of the oxide of uranium and of the potash would most probably have been the same: still, on this supposition, there ought to have been received 1.67 grm. of muriate of silver. The proportions which, next to these, would approach most nearly to the analytical result, would be when the oxygen of the oxide is to that of the potash as 5 : 4; but this would presuppose still greater differences both in the quantity of muriate of silver, and in the relative proportions of the potash and oxide of uranium.

Since the quantity as well as the number of atoms of the oxygen may in this manner be regarded as known, the information thus acquired may be applied to the analysis of other compounds of uranium.

The oxalate of uranium gave, in one experiment, from 2.67 grm. of the desiccated salt, 0.353 grm. water, 0.5835 grm. carbonic acid, and 1.7335 grm. metallic uranium. This approaches to the neutral oxalate of uranium, combined with a quantity of water of crystallization whose oxygen is thrice that of the oxide.

	By calculation.	By experiment.
• Oxide of uranium. . .	70.76 . . . . .	69.00
Oxalic acid . . . . .	16.73 . . . . .	17.99
Water . . . . .	13.51 . . . . .	13.01

Another quantity of the oxalate which I subjected to analysis appeared to be a subsalt, in which the oxygen of the oxalic acid and of the water was the same, and each double that of the oxide. But the tendency of oxide of uranium to combine with a different base is so powerful, that I do not consider this experiment as in any respect decisive.

The hydrate of oxide of uranium is readily soluble in bicarbonate of potash, and the solution after some time deposits an incrustation of lemon yellow coloured crystals, which constitute a double salt, composed of carbonic acid, oxide of uranium, and potash. When these crystals are ignited, they give off water and carbonic acid, and assume a tile red colour. Water extracts

from the residue carbonate of potash, and there remains an insoluble powder also of a tile red colour, which is a uranate of potash. 1.977 grm. of this compound dissolved in muriatic acid and precipitated with ammonia, gave 1.686 grm. oxidule = 1.716 grm. oxide, and 0.4 grm. muriate of potash, = 0.253 grm. potash. The oxygen of the oxide of uranium in this salt was therefore double that of the potash.

Uranate of potash, ignited in a current of hydrogen gas, undergoes only a partial decomposition. After this treatment, it still continues completely insoluble in water, but acids dissolve from it uranate of potash, in which, therefore, the oxide is combined with a still larger proportion of potash. The insoluble residue consists of metallic uranium.

As the uranate of barytes when precipitated by ammonia from a solution of a salt of the oxide of uranium mixed with a salt of barytes, is always accompanied with a variable quantity of uranate of ammonia, the proportion of which depends in a great measure on the relative proportions of the precipitant, and of the two salts in the original solution, I considered it preferable to form the salt by adding barytes water to a solution of nitrate of oxide of uranium. The edulcoration of this precipitate is an uncommonly tedious process, and I remarked that after having been repeatedly boiled in fresh quantities of water, it still parted with as much barytes as at first. In washing a few grammes of the salt, I expended in this manner several gallons of water before I felt satisfied that it had ceased to give off a sensible quantity of barytes. Thus prepared, the salt has a flame yellow colour, but it becomes pomegranate red after ignition. I decomposed it by solution in muriatic acid, and by precipitating the barytes by sulphuric acid, and the oxide of uranium by ammonia. From 2.128 grm. of the ignited salt, I obtained 0.451 grm. of barytes and 1.677 grm. of oxide of uranium. The oxygen of the barytes was, therefore, rather more than one-half of that of the oxide of uranium; but this excess is probably occasioned by the difficulty of washing out completely the last portions of the barytes. In the experiment in which M. Arfwedson found the oxide of uranium combined with the smallest proportion of barytes, 100 parts were combined with 16.9 parts of the earth, which, by virtue of its affinity, prevented the oxide from being decomposed by ignition. The oxygen of the barytes in this compound was, therefore, one-third of that of the oxide of uranium.

Hence it would appear, that when the oxide of uranium predominates, it combines with bases in such a proportion that its oxygen is thrice that of the base, and that when the base predominates, the oxygen of the oxide of uranium is twice that of the base. From the phenomena which take place during the treat-

ment of the uranate of potash with hydrogen gas, it is obvious that compounds exist containing a still larger proportion of base.\*

From these experiments it follows; *a.* That the quantities of oxygen combined with uranium in the oxide and oxidule are to one another in the ratio of 3 : 2. *b.* That in the neutral double salts which oxide of uranium forms with other bases, that relation by preference takes place, in which the oxygen of both bases is equal; although it might have been expected that the oxygen of the oxide of uranium, as is the case with alumina, and the oxides of iron and manganese would have been thrice that of the alkaline base with which it is associated. *c.* That oxide of uranium, when acting as an acid, contains either thrice or twice the oxygen of the base. In the first case the oxide is sufficient to saturate the base; in the second, the soluble base exists in excess.

We see also that in the analysis of a substance containing uranium, we are exceedingly liable to be deceived by the property which the oxide possesses of carrying down with it during its precipitation all insoluble bases; after the oxide has been ignited, therefore, we ought never to neglect, as M. Arfwedson recommends, to examine whether any portion of it be soluble in dilute muriatic acid, which infallibly detects the presence of a foreign base.

Uranium has a very weak affinity for sulphur. Rose has shown that the sulphuret may be prepared in the dry way by igniting the oxidule in an atmosphere of sulphuret of carbon. In the humid way it may be obtained by precipitating a salt of the oxide with hydrosulphuret of ammonia. The precipitate is black, and is soluble in an excess of the hydrosulphuret, yielding a dark brown coloured solution. Washed and dried, it forms a black heavy mass, which has all the appearance of a metallic sulphuret, but if it be digested in muriatic acid, it proves to be a mere mechanical mixture of the oxidule (which passes into solution) and sulphur. The same spontaneous decomposition and absorption of oxygen from the atmosphere take place, when the newly precipitated and still moist sulphuret is allowed to remain for a few hours upon the filter. If sulphuret of uranium prepared in the humid way, and still mixed with a small quantity of its solution in the hydrosulphuret, be exposed to the air for a number of days, its colour gradually changes to a beautiful flame yellow. The same compound may also be formed by diffusing the hydrated oxide through water, and passing through the mixture a current of sulphuretted hydrogen gas; but if after the

\* From M. Arfwedson's experiment, in which 1.057 grm. of oxide of uranium combined with 0.1733 grm. of oxide of lead, retained its oxygen in a red heat, it may be concluded that the oxide of uranium retains its oxygen in a red heat, when combined with a quantity of base whose oxygen is no more than one-sixth of the oxide of uranium.

flame yellow colour has been fully developed, we still continue to introduce the gas, it becomes deeper, and we finally obtain the ordinary dark coloured sulphuret. This flame yellow coloured substance appears to be a compound of oxide and sulphuret of uranium, an oxysulphuret: muriatic acid dissolves it, and occasions the disengagement of sulphur and sulphuretted hydrogen gas.

#### *Native Compounds of Uranium.*

Uranium occurs in the mineral kingdom in very few different states of combination. The only ores of it with which we are acquainted are: 1. The *oxidule*, in the form of pitchblende, mechanically intermixed with various metallic sulphurets and arseniurets, and with silica. From some of Klaproth's experiments, in which the silica was obtained in the state of jelly, it would appear that this mineral is occasionally a silicate of oxidule of uranium: a circumstance which still requires to be satisfactorily established. 2. The *hydrated oxide*, forming beautiful light yellow coloured masses, of a pulverulent texture, and only weakly cohering. When heated, it gives off water, and assumes a green colour; a proof that it contains no fixed saline basis. 3. *Oxide of uranium*, forming a dark yellow, sometimes brownish yellow coloured compact mineral. Ignited, it gives off water, but as it does not become green coloured, it is obvious that it must contain a fixed basis. I have detected in it lime and oxide of lead, and as I have been unable by means of the blowpipe to recognise in it any trace of phosphoric acid, I have reason to regard it as a mixture of several uranates, which probably vary both in their number and in their relative proportions. 4. The minerals styled *uranites* (uran mica), from Autun and many other localities, and from Cornwall. 5. *Sulphate of oxide of uranium*, which occurs very sparingly in Joachimsthal. From an examination, chiefly by means of the blowpipe, to which I had an opportunity of subjecting a small specimen, I find it to be a subsalt composed of sulphuric acid, and the oxides of uranium and copper. The latter oxide is probably an essential ingredient in the mineral, as is the case with the double phosphate from Cornwall.

I shall now relate the details of a minute investigation which I have made of the uranites from Cornwall and from Autun, which, on account of the similarity of their crystalline form, have been heretofore erroneously considered as constituting the same mineralogical species. The localities of this mineral are numerous, although it is never found except in very inconsiderable quantities; as, for example, in Cornwall, Autun, Johann Georgenstätt, Ebenstock, Zinnwald, Bodemais, &c. Bergmann was the first person who analyzed it. The specimen which he examined happened to be the cupreous variety from Cornwall,

and as uranium was at that time undiscovered, he was induced by his experiments to consider it a compound of muriatic acid, alumina, and oxide of copper. Hence Werner applied to it the name of chalcilite.

Klaproth, who examined the mineral in 1790, found that its solution in nitric acid is not precipitated by a solution of silver, and that it contains oxide of uranium, which he had then recently discovered, together with a quantity of oxide of copper, which, however, he regarded as accidental, because in other specimens it was totally wanting. The mineral was now regarded as a crystallized oxide of uranium.

Gregor afterwards, in 1805, examined a uran mica, in which, besides oxide of uranium, he found lime, oxide of lead, and silica; and in 1815 a green variety, from which he separated 74.4 per cent. of oxide of uranium containing a trace of oxide of lead, 8.3 per cent. of oxide of copper, and 15.4 per cent. of water. The oxide of copper he considered by far too abundant to constitute a mere accidental ingredient.

More lately, in 1819, I undertook an examination of the uranite from Antun, in which I found so considerable a proportion of lime, that it was impossible to regard it as an accidental constituent. The result of my analysis was lime 6.87, oxide of uranium 72.15, oxide of manganese and magnesia 0.80, water 15.7, gangue 2.5: hence I concluded that the mineral is a uranate of lime with water of crystallization. As I did not possess a sufficient stock of the Cornwall uranite for a regular analysis, I examined it before the blowpipe, and finding that it contained copper, that it gave off an odour of arsenic in the reducing flame, and that the globule of copper obtained with the assistance of soda was white and brittle, I concluded that the green colour is occasioned by the presence of arseniate of copper.

Towards the close of 1822, Mr. Phillips discovered phosphoric acid in the Cornwall uranite, while treating it with a caustic alkali, in order to separate the arsenic acid, which I had stated to constitute one of its ingredients. This induced him to make a new analysis of the mineral, by which he found it composed of oxide of copper 9.0, oxide of uranium 60.0, phosphoric acid 16.0, silica 0.5, water 14.5. He found also, that although the mineral is not altogether free from arsenic acid, the proportion of this ingredient is so inconsiderable, that it may be safely neglected. In consequence of his having employed erroneous numbers for the basis of his calculations, Mr. Phillips was led to regard the mineral as a mixture of 72.2 neutral phosphate of uranium with 12.3 phosphate of copper, by which there still remained unappropriated an excess of phosphoric acid. The water he regarded as shared between the two phosphates in such a manner, that the salt of uranium contains 3, and the salt of copper 2 atoms. On the whole, he considered both the Cor-

nish uranite, and that which I analyzed, to consist essentially of phosphate of oxide of uranium.

This statement of Mr. Phillips induced me to analyse the Autun uranite anew. An examination by the blowpipe instantly detected phosphoric acid, whose presence was so much the more unlooked for by me, because when in my earlier analysis I diluted the concentrated solution of the mineral in muriatic acid with alcohol, with a view to the subsequent precipitation of the lime by sulphuric acid, it did not become in the least turbid, although one would have naturally expected, that the phosphate of lime should have precipitated.

That so large a proportion of lime and oxide of copper should constitute an accidental admixture in the mineral, when, as being more powerful bases than oxide of uranium, they ought to share with it the phosphoric acid, appeared to me extremely unlikely: I thought it much more probable that the two minerals are isomorphous double salts, composed of an equal number of simple atoms, but in one of which the lime is replaced by oxide of copper. With a view to ascertain this, I undertook an analysis of both.

(A.) *Uranite from Autun.*—It is very difficult to determine the water of crystallization in this mineral with accuracy, partly because in consequence of its foliated texture, it has a peculiar tendency to absorb hygroscopic moisture, and partly because it retains only by a weak affinity even that portion of water which exists in it in a state of chemical combination. In an attempt which I made to dissolve the mineral in boiling acetic acid, I found that not a trace passed into solution, but it acquired by this treatment the same brown colour which it possesses after the water of crystallization has been expelled by ignition. When dried in a pulverised state in a temperature of  $68^{\circ}$ , it gave in many experiments between 14.4 and 15.33 per cent. of water: without this previous preparation, it gave as much as 17 per cent. The water thus expelled reacted as an alkali, and had the odour of ammonia. It was not precipitated by nitrate of silver, but it left by spontaneous evaporation traces of a crystallized salt, which was probably fluato of ammonia, for the neck of the retort was distinctly corroded in the place where the water had condensed at the commencement of the operation. I saturated with muriatic acid, and evaporated to dryness, the water expelled from three grammes of uranite, but the residue of sal ammoniac was scarcely sufficient to produce a sensible alteration on the balance.

I analyzed the mineral in three different ways:—

1. The ignited powder was dissolved in nitric acid, the solution was diluted with alcohol, and a mixture of sulphuric acid and alcohol was added, so long as any gypsum precipitated the gypsum was washed with alcohol, dried, ignited, and weighed.

To ascertain whether it had carried down any portion of oxide of uranium, it was dissolved, with the assistance of a boiling heat, in dilute muriatic acid, by which a white powder, consisting of sulphate of barytes, remained undissolved. The acid solution was precipitated neither by ammonia nor by triple prussiate of potash: the gypsum had therefore contained no oxide of uranium. The alcoholic solution was evaporated to dryness, and the residue was strongly ignited with carbonate of soda; the fused mass was then digested in water, which extracted phosphate of soda, and left a compound of oxide of uranium and soda undissolved. The latter was dissolved in muriatic acid, precipitated with caustic ammonia, washed with a solution of sal ammoniac, dried, ignited, and weighed. Muriatic acid did not dissolve from it a trace of oxide of uranium. The alkaline solution, on being saturated with muriatic acid, let fall a minute white coloured precipitate, which proved to be a mixture of neutral phosphate of oxide of uranium and phosphate of oxide of tin. The filtered liquid was boiled, in order to expel the carbonic acid gas, and precipitated by a mixture of ammonia and muriate of lime; the phosphate of lime was afterwards washed, ignited, and weighed.

2. The nitric solution of the ignited mineral was precipitated with a slight excess of acetate of lead; its yellow colour by this treatment became much paler, but did not completely disappear. The precipitate, which was a double phosphate of the oxides of lead and uranium, and which contained the whole phosphoric acid of the mineral, was washed, ignited, and weighed. It was then dissolved in nitric acid, the solution was mixed with rather more sulphuric acid than was necessary to precipitate the oxide of lead, and evaporated until most of the nitric acid was dissipated. Alcohol now added precipitated the whole of the sulphate of lead, which was washed, ignited, and weighed. The remaining liquid contained phosphoric acid and oxide of uranium, together with some sulphuric acid. Potash separated from it the oxide in the state of uranate of potash; the precipitate was dissolved in muriatic acid, and the oxide was again thrown down by ammonia, washed with a solution of sal ammoniac, and ignited. The quantity of phosphoric acid was deduced by subtracting from the weight of the double phosphate that of the oxide of lead contained by the sulphate, and that of the oxide of uranium which was equivalent to the obtained quantity of oxidule. The original solution which had been precipitated with acetate of lead, still retained about one-tenth of the oxide of uranium. The excess of oxide of lead was removed by sulphuretted hydrogen gas, and from the filtered liquid. The oxide of uranium was precipitated by ammonia. The barytes was now separated by a few drops of sulphuric acid, and the remaining liquid, after having been evaporated nearly to dryness,

was mixed with alcohol and an additional quantity of sulphuric acid, by which means the whole of the lime was obtained in the state of sulphate. From the acid alcoholic liquid, diluted with water, subphosphate of ammonia precipitated a minute portion of a mixture of magnesia and oxidule of manganese, in the form of the double ammoniacal subphosphates.

3. The barytes was separated from the nitric solution of the ignited mineral by sulphuric acid: the liquid was then concentrated, and the lime was precipitated by a mixture of sulphuric acid and alcohol. The filtered solution was freed from alcohol by evaporation, and decomposed by ammonia. The precipitate, which was a double subphosphate of oxide of uranium and ammonia, was very cautiously calcined (in a high temperature its colour is partially converted to a green<sup>\*</sup>), weighed, and decomposed by ignition with potash; and the oxide of uranium was afterwards isolated by the process which has been already described. Its weight, deducted from that of the calcined phosphate, indicated that of the phosphoric acid, I found to my surprise that subphosphate of ammonia still detects the presence of magnesia and oxidule of manganese in a liquid from which phosphate of oxide of uranium has been precipitated by ammonia.

The following are the results of three analyses of 100 parts of the ignited uranite, performed according to the three foregoing methods:

	A.	B.	C.	Mean.
Barytes.....	1.84	1.83	1.72	1.80
Lime .....	6.75	6.84	6.56	6.72
Magnesia .....	—	0.23	0.22	0.23
Oxidule of mangan. }				
Oxide of uranium ..	71.25	70.05	69.98	70.43
Phosphoric acid. ..	16.75	18.87	16.44	17.36
Oxide of tin .....	0.06	—	—	0.06
Gangue. ....	3.35	2.53	2.40	2.79
			(2.50 ?)	
	100.00	100.35	97.42 (?)	99.39

Admitting that the mean of these three analyses approaches nearest to the truth, and that the water amounts to 14.9 per cent. (which however includes the fluoric acid and ammonia), it will follow that 100 parts of uranite are composed of

\* This decomposition does not take place with the double subsalt of lime.

		Atoms.
Barytes. ....	1.51 containing oxygen	0.160 .. 1
Lime .....	5.66	1.585 .. 1
Magnesia. ....	} 0.19	0.07
Oxide of mangan. ....		
Oxide of uranium. . .	59.37	3.128 .. 2
Phosphoric acid. ....	14.63	8.193 .. 5
Water .....	14.90	13.246 .. 8
Gangue. ....	2.70	
Fluoric acid. ....	} Trace	
Ammonia. ....		
	99.06	

It follows from this that the oxygen of the oxide of uranium is double that of the lime; that the sum of the oxygen of all the bases is to that of the phosphoric acid as 3 : 5; and that the oxygen of the water is eight times that of the lime. The quantity of water actually found rather exceeds this proportion, but the slight excess is probably in combination with the salts of lime and barytes, and with the fluoric acid and ammonia. This salt is, therefore, composed of 3 atoms of subphosphate of lime on the same degree of saturation as in crystallized apatite), 1 atom of subphosphate of oxide of uranium, and 48 atoms of water, mechanically intermixed with very minute quantities of the phosphates of iron, magnesia, and oxide of manganese.

Concentrated sulphuric acid develops the fluoric acid more decisively than ignition, but the quantity is in both cases so inconsiderable, that it cannot be regarded otherwise than an accidental admixture, as indeed appears to be the case with it in all the native phosphates.

(B.) *Uranite from Cornwall.* - The small stock which I possessed of this mineral permitted me to subject it only to a single analysis; but my results agree so closely with those of Gregor and Phillips, that a repetition of it would perhaps be superfluous.

One gramme, previously pounded and dried in a temperature of 68°, lost by ignition in a small glass retort 0.1505; the expelled water was neither acid nor ammoniacal, and was in all respects pure. The yellow coloured residue was fused with carbonate of soda, and the acid and alkali were extracted with water. The oxides thus separated had a dark green colour, and left by solution in muriatic acid and supersaturation with carbonate of ammonia, an insoluble matter, weighing 0.007 gram. which contained silica, alumina, and a distinct trace of tin or lead. The liquid was concentrated until the whole of the ammonia was expelled, the precipitated oxides were then dissolved in muriatic acid, and the copper was thrown down by

sulphuretted hydrogen gas. The sulphuret of copper weighed after roasting 0.094 grm. Dissolved in muriatic acid\* and mixed with murate of barytes, it gave 0.028 grm. sulphate of barytes, = 0.0096 grm. sulphuric acid; consequently the oxide of copper amounted to 0.0844 grm. The liquid which had been treated with sulphuretted hydrogen was boiled in order to expel the excess of gas, and precipitated by ammonia. The precipitate, washed with a solution of sal ammoniac and ignited, gave 0.592 grm. oxide of uranium, = 0.6025 grm. oxide.

The alkaline liquid, which contained the acid of the uranite, was supersaturated with muriatic acid, and precipitated by sulphuretted hydrogen gas: some sulphuret of arsenic separated, but too inconsiderable to be weighed. The portion of green uranite on which my original blowpipe examination was made, contained a considerably larger quantity of arsenic acid: as this acid is isomorphous with phosphoric acid, its proportion in different specimens will probably be liable to great variations. The uranite was therefore composed of

	Atom.
Oxide of copper . . . . . 8.44 containing oxygen	1.702 .. 1
Oxide of uranium. . . . . 60.25	3.175 .. 2
Phosp. ac. with ars. ac. 15.56*	8.72 .. 5
Water . . . . . 15.05	13.24 .. 8
Gangue. . . . . 0.70	
	-----
	100.00

Here we find the same multiples as in the uranite from Autun, with the difference that the lime is replaced by a quantity of oxide of copper, containing the same amount of oxygen. The quantity of phosphoric acid is rather too high, because there is included under it both the arsenic acid and the necessary loss of the analysis. This uranite contains also fluoric acid, perhaps more abundantly than the specimens from Autun, but the lime and barytes are wholly wanting.

Both these uranites are therefore double subphosphates of oxide of uranium, the one with oxide of copper, and the other with lime. As lime and oxide of copper, according to Mitscherlich's admirable discovery are isomorphous, they must, when combined with an equal number of atoms of oxide of uranium, phosphoric acid, and water, assume the same crystalline form; and, therefore, the two minerals will be considered as identical, by those who confine themselves to the crystalline form, in establishing a mineralogical species, although this arrangement, with reference to their composition, is erroneous.

\* Determined from the loss.

As they deserve to be distinguished by different names, I propose to call the Cornwall variety *chalcolite*, the appellation which it had previously received from Werner, and to appropriate to the yellow variety the name of *uranite*.

## ARTICLE V.

*Observations on a Safety Hood and Mouth-piece, invented by John Roberts, for enabling Persons to enter Apartments in Cases of Fire, to effect Measures for its early Extinction, and for the Removal of Goods, Papers, &c.* By W. R. Whatton, Esq. F.A.S. &c.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

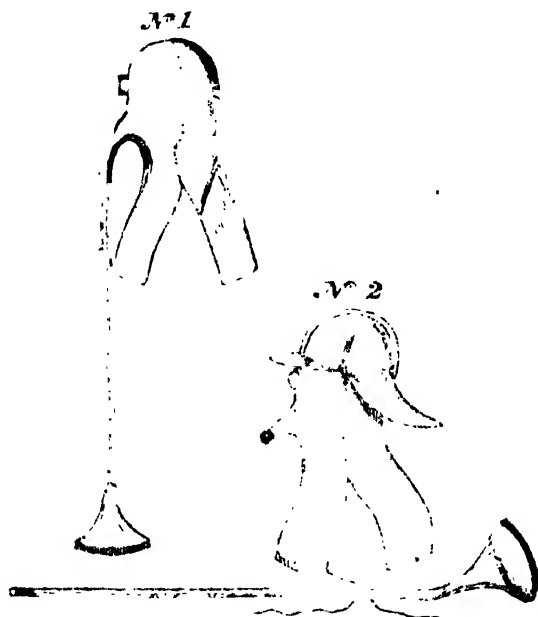
I do myself the honour of offering you, for insertion in your highly valuable journal, some short observations on a recent invention brought before the public in this town, by John Roberts, a poor working mason, of St. Helens, in Lancashire, which, after being submitted to the test of experiment, has answered the expectations formed of its apparent utility in the highest degree.

The construction of the instrument is very simple, and consists of a hood or covering for the head and neck, made of strong leather, and closely adapted to the upper part of the body; into the front or face part of which is inserted a sight piece of glass, set in a projecting square tin socket or frame; and a mouth-piece, to which is attached an open tube, constructed of spiral wire covered with leather, descending to within about nine inches of the floor, steadied by a strap and buckle round the leg, and having at its end a large funnel or trumpet-shaped opening of tin, closed with strong flannel or horse-rug, and carefully lined on its inner surface with a layer of sponge.

The principle upon which its merits appear entirely to rest, is the facility afforded to the wearer of respiring freely a much less impure and a comparatively cool atmospheric air, in places filled with smoke and vapour, arising from the combustion of matter of any kind, and deleterious, in a greater or less degree, to animal life.

To render the hood fit for immediate use, nothing more is necessary than to moisten thoroughly the flannel and sponge at its lower end with water; taking care to free it from any superfluous quantity of fluid which it may have imbibed, and which would otherwise interfere with the free passage of air through the instrument, and impair its usefulness. The manner of its operation is to enable the wearer to inhale the air from the

lowest stratum of the apartment, where it is always the least impure, to filtrate and cool it by transmission through the breathing funnel, and thus to deprive it of the smoke and charcoal, given off by the combustion, with which it is strongly impregnated.



No. 1, Roberts's invention as given by himself.

No. 2, The same, with the suggested improvements.

For these purposes the principle of Roberts's instrument is well adapted, and its construction well imagined and invented; but several important improvements might be made which would go far towards the completion of the machine, and which immediately suggest themselves on a bare examination, or on its application to experiment.

The first alteration I would recommend is the insertion of a screw joint into the upper part of the tube, at about two or three inches from its origin, furnished with a washer of leather, and made air-tight. This simple addition would secure two important advantages: first, it would enable the person using the hood to relieve himself from exhaustion and oppression on coming out of the apartment, by the instant admission and respiration of pure atmospheric air, without incurring loss of time by removing the whole hood, as is the case on its present construction; and

secondly, it would prevent all danger from a sudden exposure of the breast and neck (already in a state of profuse perspiration) from a high temperature to cold air, caused by opening the dress to loose the straps and buckles by which the machine is fastened round the shoulders and chest.

The second alteration would be to insert a shallow oblong concavo-convex glass, instead of the square and socket at present in use; by which the man would be enabled to see on all sides with much less difficulty, and without any change of posture; while, at the same time, refraction would be prevented by the thinness of the medium, which cannot be the case with a plano-convex glass, as by some gentleman has been recommended. Over this sight-piece might also be advantageously fixed a strong leather peak, to protect the eyes from the glaring light of the fire, and the glass from injury by falling bodies, or blows from above, and in lieu of the simple water for the immersion of the breathing funnel, a solution of caustic potash might be substituted, which would neutralize the carbonic acid gas and wood acid, suspended in the smoke (the contact of which last is so severely painful to the eyes), and thereby, in a still greater degree, effect the advantages for which the machine is intended.

In the experiments of Friday last, made before the President and Directors of the Manchester Fire and Life Assurance Company, and a number of scientific gentlemen, the apartment in which Roberts remained 24 minutes, at a temperature of 130° Fahr. was a foundry drying oven, filled with smoke and vapour, given off by the combustion of damp cotton, waste wet hay, and sulphur, the least exposure to which produced intense smarting in the eyes, and an excessive irritation on the lungs, and would probably have destroyed life if it had been continued but for a few minutes;\* while to a person wearing the apparatus nothing more occurred than an acceleration of the action of the heart from 70 to about 160 per minute, attended by a free perspiration over the surface of the body; with some sensation of exhaustion and giddiness in those unaccustomed to the experiment.

The chief excellence of Roberts's instrument is its simplicity, for it can never be admitted that an invention however efficient in the purposes for which it is intended, if it be complex in its construction, and difficult in its application, can be at all comparable in value to one so plain in principle, and so intelligible in its utility, as his is.

It must likewise be borne in mind that the machine is not invented for scientific persons, or for those who are familiar

\* On submitting this mixed air to chemical analysis, it was found, in the several experiments of Dr. Henry and Mr. Dalton, to be atmospheric air charged with about one per cent. of carbonic acid gas, and deprived of a correspondent volume of oxygen, and impregnated with wood acid and some essential oil.

with experiment; but for men of inferior capacities and no education, who, in the fulfilment of even plain directions in the hour of danger and hurry, are often likely to misunderstand their meaning, and defeat their intention.

Taking therefore these circumstances into consideration, it is my opinion that the invention is amply equal to the purposes for which it is intended, and highly deserving the confidence and encouragement of a liberal public.

Roberts has received from the Manchester Assurance Company a premium of 50 guineas, and their permission to use any documents respecting the invention which have been laid before the Board of Directors; and I should feel myself at all times complimented by any application for further information, or indeed by any suggestion likely to forward the poor fellow's interest, or to make the invention more generally known and useful.

I have the honour to be, Gentlemen,

Your very obedient servant,

W. R. WHATTON,

Surgeon to the Manchester Fire and Life Assurance Company.

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Might not *talc* be advantageously substituted for glass for the sight-holes, as the danger of the latter substance's breaking from sudden exposure to intense heat would thereby be avoided?—*Edit.*

## ARTICLE VI.

*Chemical Examination of two Specimens of the Soil of the Cavern of Kühloch.* By M. Chevreul. (Communicated by the Rev. W. Buckland, Professor of Mineralogy in the University of Oxford, &c.) *To which is prefixed an Account of the Cave, from the Reliquiæ Diluvianæ.*

THE cave of Kühloch is more remarkable than the rest, as being the only one I have ever seen, except that of Kirkdale, in which the animal remains have escaped disturbance by diluvial action; and the only one also in which I could find the black animal earth, said by other writers to occur so generally, and for which many of them appear to have mistaken the diluvial sediment in which the bones are so universally imbedded. The only thing at all like it that I could find in any of the other caverns, were fragments of highly decayed bone, which occurred in the loose part of the diluvial sediment in the caves of Scharzfeld and Gailenreuth; but in the cave of Kühloch, it is far otherwise. It is literally true that in this single cavern (the size and proportions of which are nearly equal to those of

the interior of a large church), there are hundreds of cart-loads of black animal dust entirely covering the whole floor to a depth which must average at least six feet, and which, if we multiply this depth by the length and breadth of the cavern, will be found to exceed 5000 cubic feet. The whole of this mass has been again and again dug over in search of teeth and bones, which it still contains abundantly, though in broken fragments. The state of these is very different from that of the bones we find in any of the other caverns, being of a black, or, more properly speaking, dark milder colour throughout, and many of them readily crumbling under the finger into a soft dark powder resembling mummy powder, and being of the same nature with the black earth in which they are imbedded. The quantity of animal matter accumulated on this floor is most surprising, and the only thing of the kind I ever witnessed; and many hundred, I might say thousand individuals must have contributed their remains to make up this appalling mass of the dust of death. It seems in great part to be derived from comminuted and pulverized bone; for the fleshy parts of animal bodies produce by their decomposition so small a quantity of permanent earthy residuum, that we must seek for the origin of this mass principally in decayed bones. The cave is so dry that the black earth lies in a state of loose powder, and rises in dust under the feet: it also retains so large a proportion of its original animal matter, that it is occasionally used by the peasants as an enriching manure for the adjacent meadows.\* The exterior of this cavern presents a lofty arch in a nearly perpendicular cliff, which forms the left flank of the gorge of the Esbach, opposite the castle of Rabenstein. The depth of the valley below it is less than 30 feet, whilst above it the hill rises rapidly, and sometimes precipitously to 150 or 200 feet. This narrow valley or gorge is simply a valley of denudation, by which the waters of the Esbach fall into those of the Weissen. The breadth of the entrance arch is about 50 feet, its height 20 feet. As we advance inwards, the cave increases in height and breadth, and near its inner extremity divides into large and lofty chambers, both of which terminate in a close round end, or *cul de sac*, at the distance of about 100 feet from the entrance. It is intersected by no fissures, and has no lateral communications connecting it with any other caverns, except one small hole close to its mouth, and which opens also to the valley. These circumstances are important, as they will assist to explain the peculiarly undisturbed state in which the interior of this cavern

\* I have stated that the total quantity of animal matter that lies within this cavern cannot be computed at less than 5000 cubic feet; now allowing two cubic feet of dust and bones for each individual animal, we shall have in this single vault the remains of at least 2500 bears, a number which may have been supplied in the space of 1000 years by a mortality at the rate of two and a half per annum.

has remained, amid the diluvial changes that have affected so many others. The inclination of the floor for about 30 feet nearest the mouth is very considerable, and but little earth is lodged upon it; but further in, the interior of the cavern is entirely covered with a mass of dark brown or blackish earth, through which are disseminated, in great abundance, the bones and teeth of bears and other animals, and a few small angular fragments of limestone, which have probably fallen from the roof, but I could find no rolled pebbles. The upper portion of this earth seems to be mixed up with a quantity of calcareous loam, which, before it had been disturbed by digging, probably formed a bed of diluvial sediment over the animal remains; but, as we sink deeper, the earth gets blacker and more free from loam, and seems wholly composed of decayed animal matter. There is no appearance of either stalactite or stalagmite having ever existed within this cavern.

"In some of the particulars here enumerated, there is an apparent inconsistency with the phenomena of other caverns, but the differences are such as arise from the particular position and circumstances of the cave at Kühloch: the absence of pebbles, and the presence of such an enormous mass of animal dust, are the anomalies I allude to; and both these circumstances indicate a less powerful action of diluvial waters within this cave than in any other, excepting Kirkdale. To these waters, however, we must still refer the introduction of the brown loam, and the formation or laying open of the present mouth of the cavern: from its low position so near the bottom of the valley, this mouth could not have been exposed in its present state, and indeed must have been entirely covered under solid rock till all the materials that lay above it had been swept away, and the valley cut down nearly to its present base; and as the cave ends inwardly in a *cul de sac*, and there is no vertical fissure, or any other mode of access to it, but by the present mouth, if we can find therein any circumstances that would prevent the admission of pebbles from without, or the removal of the animal remains from within, the cause of the anomaly we are considering will be explained. That the throat of the cave by which we ascend from the mouth to the interior is highly inclined upwards, so that neither would any pebbles that were drifting on with the waters that excavated the valley ascend this inclined plane to enter the cave, nor would the external currents, however rapidly rushing by the outside of the mouth, have power to agitate (except by slight eddies in the lower part of the throat) the still waters that would fill the body of the cavern, and which

from the mud suspended in them upon the undisturbed remains of whatever kind that lay on the floor. From its low position, it is also probable that the vault formed the deepest recess of an

extensive range of inhabited caves, in which successive generations of antediluvian bears withdrew themselves from the turbulent company of their fellows, as they felt sickness and death approaching; the habit of domesticated beasts and birds to hide themselves on the approach of death renders it probable that wild and savage animals also do the same. The unusual state of decay of the teeth and bones in this black earth may be attributed to the exposed state of this cavern, arising from its large mouth and proximity, to the external atmosphere and to the absence of that protection, which in closer and deeper caves they have received by being secluded from such exposure, or imbedded in more argillaceous earth, or inverted with, and entirely sealed up beneath a crust of stalagmate."—(*Buckland's Reliquiæ Diluvionæ.*)

*Analysis by M. Chevreul.*

Mr. Buckland transmitted to me through Mr. Underwood two specimens of the soil of the cavern of Kùhloch taken at different depths in order that I might analyse them; this cavern contains a great number of fossil bones, belonging to carnivorous and herbivorous animals, which Mr. Buckland conceives were not transported by water into the situation in which they are now seen, but that the Kùhloch cavern was the haunt of carnivorous animals which died there, and then fossil bones are now found in a state of greater or less decay according to the degree of exposure to the atmosphere that they have undergone.

The letter A denotes a specimen of the soil taken at the depth of two feet, B one at six feet below the surface.

Both the specimens are, in great measure, in a pulverulent state, containing small masses which easily crumble to pieces; their colour is orange brown, pretty much like that of some bog iron ores (*mines de fer hydratées limonneuses*); the colour resides principally in the finest particles, as is evident if we agitate the specimens in water, and decant the fluid before it has become clear; the pulverulent particles remain suspended, while a granular sandy matter subsides of a yellowish grey colour; when a deposit has formed from the muddy water which had been decanted off, it is found to have a fine orange yellow colour. The specimen A contains a smaller proportion of pulverulent particles than B, and is also less coloured.

Previous trials having shown that the matter soluble in water was in part alterable by the action of heat, like organic substances, I submitted both specimens to two series of experiments, to determine first the nature of the substances indestructible by heat, and secondly that of the matter destructible by that agent.

## § 1.—Experiments on the Indestructible Matter of each Specimen.

*Experiments on Specimen A.*

1. 2 grammes,\* exposed to a temperature of  $212^{\circ}$  to  $250^{\circ}$ , lost 0.185 gr. of water.

2. The dried matter, heated in a platina capsule, first exhaled an ammoniacal, oily and hydrocyanic odour; it then kindled like a pyrophorus at a scarcely red heat, and gave off sulphurous acid, and afterwards ceased to glow. It lost in this operation 0.165 gr.

3. The calcined matter weighed 1.650 gr.; it dissolved in part in weak nitric acid, with effervescence; the residuum weighed 0.228 gr.; it was sandy and coloured, it was separated by the filter.

*I. Sandy Residuum.*

4. Ignited with potash in a silver crucible, the fused mass was not coloured green; it was dissolved in water and muriatic acid; by evaporation and water 0.159 gr. of silica was separated from the solution.

5. The liquid from which the silica had been separated gave a precipitate, which, by means of potash, afforded 0.026 gr. of alumina, and 0.013 gr. of peroxide of iron.

6. The ammoniacal liquid mixed with oxalic acid gave oxalate of lime, equivalent\* to 0.005 gr. of lime.

*II. Nitric Solution.*

Precipitated by ammonia and the precipitate washed

*Experiments on Specimen B.*

1. 2 grammes lost at the same temperature 0.210 of water.

2. Same phenomena as the preceding (2): the loss = 0.2 gr.

3. Same phenomena. sandy residuum = 0.237 gr.

*I. Sandy Residuum.*

4. Same phenomena; silica = 0.185 gr.

5. Same phenomena; alumina = 0.04 gr.; peroxide of iron = 0.013 gr.

6. Same phenomena; lime = 0.02 gr.

*II. Nitric Solution.*

7. Same phenomena; precipitate = 0.635 gr.

\* The weights are in grammes; the degrees of heat are those of Farenheits' scale.

with tepid water, filtered, and calcined, exhaled a slight sulphureous odour; it had a reddish yellow colour, and weighed 0.505 gr.

*a. Precipitate of No. 7.*

8. 0.2 gr. of this precipitate gave no indications of manganese by fusion with potash. The residuum dissolved in nitric acid, and treated with solution of potash, gave no alumina to the alkali.

9. 0.225 gr. of the same precipitate, treated with sulphuric acid and alcohol, gave a residuum and an alcoholic solution.

10. The residuum consisted of sulphate of lime 0.275 gr. magnesia 0.010 gr.

11. The alcoholic solution was mixed with water, and slowly concentrated, and the residuum diluted with water, precipitated by ammonia, and filtered.

The filtered liquid evaporated to dryness left phosphoric acid, whose nature was ascertained by means of oxide of silver and oxide of lead.

The precipitate dried in the air weighed 0.1 gr.; heated to  $212^{\circ}$  water and ammonia were disengaged; it dissolved entirely after calcination in weak sulphuric acid; the solution, treated with boiling subcarbonate of potash in excess, yield flocculi of carbonate of magnesia, and peroxide of iron; the alkaline liquid probably contained phosphoric acid; neutralized by nitric acid and evaporated to dryness, a minute portion of matter remained of a light rose colour.

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*a. Precipitate of No. 7.*

8. 0.335 gr. of this precipitate gave with potash a sensible quantity of oxide of manganese; the alumina, if it contained any, was in extremely small quantity.

9. 0.3 gr. of precipitate (7) similarly treated.

10. *Residuum* wholly formed of sulphate of lime = 0.325 gr.

11. *Alcoholic solution*, similarly treated.

*Filtered liquid*, same results.

*Precipitate dried in the air* = 0.075 gr. exposed to  $212^{\circ}$ , gave off water and ammonia; carbonate of potash took up phosphoric acid, and left a residuum consisting of magnesia, peroxide of iron, and oxide of manganese.

We may conclude from these experiments that the precipitate consisted of the phosphates of lime, and magnesia, and of peroxide of iron, probably united to phosphoric acid.

*b. Solution of No. 7, from which the Precipitate (7) had been separated.*

12. Oxalic acid separated 0.362 gr. of lime = 0.872 gr. sulphate of lime, which, when dissolved in muriatic acid, gave no cloudiness with ammonia, but the solution assumed a violet rose colour.

13. The liquid which had been precipitated by oxalic acid was mixed with carbonate of potash, and evaporated to dryness; the residuum, treated with water, gave carbonate of magnesia, = 0.130 gr. of dry magnesia; it contained a trace of silica.

14. The solution from which the magnesia had been separated contained 0.014 gr. of sulphuric acid, which was separated by nitrate of barytes; it contained no phosphoric acid.

15. It is very probable that in the specimen A, calcined,

1st. The 0.014 gr. of sulphuric acid were united to 0.01 of lime.

2d. That the 0.362 gr. of lime precipitated by oxalic acid, minus 0.01 gr. and the 0.130 gr. of magnesia were in the state of carbonates.

According to the preceding experiments, the specimen A contained,

We may conclude from these experiments that the precipitate (7) was formed of the phosphates of lime and magnesia, and of the oxides of iron and manganese, probably united to phosphoric acid.

*b. Solution of No. 7, from which the Precipitate (7) had been separated.*

12. Oxalic acid threw down 0.270 gr. of lime; the lime combined with sulphuric acid, and then dissolved in nitric acid, gave no rose colour with ammonia.

13. The liquid which had been precipitated by oxalic acid gave 0.06 gr. of magnesia, and 0.02 gr. of silica.

14. The liquid from which the magnesia and silica had been separated contained 0.016 gr. of sulphuric acid, without any phosphoric acid.

15. It is very probable that in the specimen B, calcined,

1st. The 0.016 gr. of sulphuric acid was combined with 0.011 gr. of lime.

2d. That the 0.27 gr. of lime, minus 0.011 gr. and the 0.60 of magnesia, were in the state of carbonates.

According to the preceding experiments, the specimen B contained,

Grammes.		Grammes.	
Water, and matter volatile at 250°. . . . .	0.185	Water, and matter volatile at 250°. . . . .	0.215
Matter volatilized by combustion and a red heat . . . . .	0.165	Matter volatilized by combustion and a red heat . . . . .	0.200
Sandy Silica . . . . .	0.159	Sandy Silica . . . . .	0.185
residuum. < Alumina . . . . .	0.026	residuum. < Alumina . . . . .	0.040
Peroxide of iron . . . . .	0.013	Peroxide of iron . . . . .	0.013
Lime . . . . .	0.005	Lime . . . . .	0.002
Phosphates of lime } — magnesia } — iron ? }	0.505	Phosphates of lime } — magnesia } — iron ? }	0.635
Carbonate of lime . . . .	0.624	Carbonate of lime . . . .	0.459
— magnesia . . . . .	0.268	— magnesia . . . . .	0.124
Sulphate of lime . . . . .	0.024	Sulphate of lime . . . . .	0.027
		Silica . . . . .	0.020
	1.974		1.920
Loss . . . . .	0.026	Loss . . . . .	80
	2.000		2.000

16. The loss must be rather greater in reality than is indicated in the preceding tables, because the carbonates of lime and magnesia must have lost a portion of their carbonic acid by calcination; but the effervescence produced during the solution of the calcined matters in nitric acid (3) and (3), proves that the whole of the carbonic acid had not been volatilized by the calcination. I should add, that I looked in vain for fluoric acid in the soil of the cavern of Kühloch.

## § 2. *Series of Experiments to determine the Nature of the Matter of Specimens A and B which is alterable by Heat.*

### *Experiments on Specimen A.*

17. 10 parts of the specimen A were treated at least 20 times with 100 parts of water at each operation. The first washings were made at the temperature of 140° to 176° Fahr.; the last were at the boiling point. The washings mixed and evaporated left an orange coloured precipitate weighing 0.66 parts. The matter insoluble in boiling water will be examined in Article 2.

### *Experiments on Specimen B.*

17. 10 parts of the specimen B treated in the same manner gave a residuum weighing 1.33 part, which differed from the preceding only in being of a more red orange colour, and containing a little more deliquescent matter. The matter insoluble in boiling water will be examined in Article 2.

18. The analogy apparent between the two residues of the specimens A and B, and especially the small quantity of the substance which I was possessed of, determined me to mix them. I shall relate the experiments that I made upon these residues, which I shall describe as the *soluble matter* of the soil of the cavern of Kühloch.

19. The soluble matter was four times treated with ten times its weight of cold water. The washings were evaporated to dryness, and the residue was treated with alcohol; the alcoholic solution was evaporated, and the residue treated with fresh alcohol: by these means the soluble matter was divided into *alcoholic extract*, and *residuum insoluble in alcohol*, which last was added to the matter undissolved by cold water in the four washings above mentioned.

20. I shall now examine successively,

Art. 1, the matter of the soil soluble in boiling water, divided into	} 1st, alcoholic extract; 2d, residuum undissolved in alcohol.
Art. 2, the matter of the soil undissolved in boiling water.	

#### ARTICLE I.—*Examination of the Matter of the Soil of Kühloch dissolved in Boiling Water.*

##### 1. *Alcoholic Extract.*

21. It contained an organic principle of a red orange colour, an acidulous ammoniacal salt, traces of phosphate of magnesia, of chloride of potassium, and of a salt of potash, the acid of which was of an organized nature. This extract was deliquescent; by distillation it yielded a very acid product, the strong smell of which resembled that of the hydrocyanic acid; having only 0.03 gr. of alcoholic extract, I could not make any further experiments.

##### 2. *Matter insoluble in Alcohol.*

22. I subjected it to the action of cold water in the mode described in my *Considerations sur l'Analyse Organique et sur ses Applications*, p. 119. The first solutions were of an orange colour, and the last yellow; from these solutions there were obtained, first, some lamellar transparent crystals; secondly, some smaller crystals in fine needles, and of a straw colour.

##### 1st. *Lamellar Transparent Crystals.*

23. They were slightly coloured, but the colour was unequally distributed; there can be no doubt but that if they had been perfectly pure, they would have been colourless; they suffered no alteration by means of concentrated sulphuric acid; potash disengaged much ammonia; they precipitated sulphate of barytes from the nitrate; they very slightly reddened litmus paper: the precipitate which they gave with acetate of lead

examined with the blowpipe gave a small quantity of phosphate only; lastly, the solution of these crystals precipitated neither oxalate of ammonia nor acidulous nitrate of silver.

24. When exposed\* in a glass tube to a gentle heat they decrepitated and effloresced, losing water and some ammonia; at a higher temperature they yielded ammonia, mixed with an hydrocyanic odour, a little sulphuretted hydrogen and sulphur, and a large proportion of sulphite of ammonia, which crystallized as it cooled in long needles; this salt was recognised by its taste; by the sulphurous gas which was evolved by muriatic acid; and lastly, by the yellow precipitate which it gave with sulphate of copper. The residue of the distillation was entirely soluble in water; this solution when concentrated was slightly acid; but when strongly calcined, it became slightly alkaline; nevertheless it did not effervesce while dissolving in nitric acid; this solution precipitated platina in the state of a double salt of potash, and with the nitrate of barytes it gave sulphate. I conclude from these experiments that these lamellar transparent crystals were sulphate of ammonia and potash, a double salt first described by Link in 1796.

*Crystals in Fine Needles, and of a Straw Colour.*

25. Examined with a glass they appeared to be formed of several kinds of substances; unfortunately I had too few to attempt a separation of them.

26. These crystals put in excess into distilled water of the temperature of 52° Fahr. gave a solution of a pale yellow colour, consisting of water 199 parts and 1 of crystals. This solution was rendered slightly turbid by heat; gave an abundant precipitate of sulphate of barytes with the nitrate, and of oxalate of lime with the oxalate of ammonia; it gave no chloride of silver with the nitrate; lastly, these crystals were not altered by sulphuric acid, and yielded ammonia with potash.

27. 0.137 gramme of the crystals yielded by distillation, first, ammoniacal water, having an hydrocyanic smell; secondly, sulphur; 3dly, sulphate of ammonia; fourthly, a residuum, which, after incineration, weighed 0.122 gramme: this dissolved in nitric acid without effervescence, except a few flocks of silica: ammonia precipitated 0.007 gramme of a matter which appeared to be phosphate of lime, magnesia, iron, and manganese; there remained 0.112 gramme of sulphate of lime in solution.

28. It is evident that the yellow needle-form crystals contained a large proportion of sulphate of lime and a small quantity of yellow colouring principle; but was this sulphate simply mixed with the sulphate of ammonia and potash, or was in combination with the sulphate of ammonia? or, lastly, was it mixed with the

ammoniac-magnesian-phosphate? These questions I cannot answer.

29. As to that part of the matter insoluble in alcohol which was not dissolved by cold water (22), it consisted of sulphate of lime, phosphate of lime and magnesia, silica, and yellow colouring principle.

30. It is not impossible that there were other substances besides those which I have mentioned, in the matter of the soil of K  hloch, soluble in boiling water; but the small quantity of the substance which I possessed did not allow of my ascertaining whether any such really existed in it.

#### ARTICLE II.—*Examination of the Matter undissolved by Boiling Water.*

31. The portions of the specimens A and B undissolved by boiling water were both separated by means of levigation (elutriation !) into *pulverulent matter* and *sandy matter*.

32. The pulverulent matter of specimen A differed from that of B in being of a lighter colour; they were both of an amber tint, and were mixed together.

33. The sandy portions of the two specimens being perfectly similar in their physical properties, they were also mixed.

##### 1. *Pulverulent Matter.*

34. It did not sensibly impart colour to cold water, but gave a slight yellow tint to boiling water; it was repeatedly treated with boiling alcohol.

35. The first alcoholic washings filtered while hot deposited a flocculent substance on cooling; the washings being mixed were evaporated nearly to dryness, a nearly colourless substance was obtained which was partly fusible; it was treated with a hot solution of potash, and this liquid was afterwards filtered.

36. This alkaline solution was neutralized while hot by muriatic acid; some drops of oily matter were obtained which solidified on cooling; this substance appeared to be stearic or margaric acid; for it was dissolved by hot solution of potash, and which, when mixed with cold water, deposited a pearly substance.

37. The residuum insoluble in potash (35) was treated with hot alcohol and some flocculent azotized matter (*mat  re azot  e*) was separated, and a fatty matter dissolved which melted into perfectly limpid and colourless drops, and solidified like bees wax; this substance, when boiled with a solution of potash, formed an emulsion rather than a solution: it is probable that it consisted of fatty saponifiable and saponified matter, and some which was not saponifiable.

38. The pulverulent matter which had been washed with

alcohol produced a brisk effervescence when treated with weak hot muriatic acid. The solution was filtered when cold, and the matter remaining upon the filter was washed with water until nitrate of silver showed that it contained no muriatic acid; the filtered liquor resembled muriate of platina in colour. The washings which contained a notable excess of acid were slightly coloured; while the latter which contained none were of a deep colour, which proves, that the colouring organic matter when uncombined, is more soluble in pure water, than in that which is acidulated.

39. The muriatic solution upon the addition of a little water deposited a red crystalline matter, resembling that of deutoxide of manganese; the solution was filtered, and the red powder washed.

When subjected to distillation, this red powder yielded an acid water, and afterwards oily ammonia; a sulphurous product, and afterwards a quantity of charcoal which left only a trace of non-effervescing ferruginous ash, probably containing alumina. This powder, when heated in the air, burnt, giving out sparks like the charcoal which is set on fire by nitro-sulphuric acid.

The filtered muriatic solution was mixed with muriate of barytes; there was no sensible precipitation in 24 hours; nevertheless I filtered the liquor twice, and evaporated it to dryness. The residuum when distilled gave a sulphurated product; and when it was calcined with nitrate of barytes, and the whole again treated with nitric acid, sulphate of barytes was obtained. The soil of Kühloch, therefore, contains some sulphur which is not in the state of sulphuric acid. Is it in that of an hyposulphate, or of sulphuretted hydrogen combined with organic matter? or is it in combination with organic matter without being in the state of an acid? These are points which I cannot decide.

40. The matter undissolved by muriatic acid (38) was treated with boiling alcohol, and the solution filtered.

41. This solution did not contain any sensible quantity of fatty matter, for the residue of evaporation was totally soluble in cold water. This residue when distilled yielded an acid water, a trace of sulphurated product, a thick brown matter, some sub-carbonate of ammonia and charcoal.

42. The residue undissolved by alcohol (40) consisted of silica, alumina, magnesia, peroxide of iron, a trace of oxide of manganese,\* yellow colouring matter, some alcoholic extract

\* Having burnt this residuum, I obtained a cinder, which was partly dissolved by nitric acid without effervescence. The solution was precipitated by ammonia, and the precipitate consisted of alumina and peroxide of iron. The filtered solution was of a purplish rose colour, resembling that of the sulphate of lime obtained from the specimen A (12); at first I attributed this colour to manganese; to determine this I evaporated the solution to dryness, and obtained a black powder mixed with nitrate of magnesia; the latter dissolved in water, but the former did not. The black powder contained iron and a trace of manganese, but the small quantity of caneleon obtained by heating the powder in potash, induced me to think that there was another matter which escaped me.

(21), and azotized organic matter, which had probably suffered alteration. I have every reason to think that the two last substances formed a kind of *lake* with the alumina and peroxide of iron. The residuum heated in contact with the air burnt like a pyrophorus, and the cinder which it left was much less coloured than itself. By distillation it yielded a slightly acidulous water, oil, sulphuretted hydrogen without sulphite of ammonia, carbonate of ammonia, a product possessing the smell of hydrocyanic acid, but which did not produce prussian blue; and lastly, a bulky black residuum.

## 2. *Sandy Matter.*

43. The nature of the sandy matter was analogous to that of the pulverulent matter, with this difference, that it contained a larger proportion of phosphates, and a smaller proportion of yellow organic matter, alumina, and oxide of iron. It burnt when heated in the air, but not so well as the pulverulent matter. By distillation, it yielded ammoniacal water, sulphuretted hydrogen, carbonate of ammonia, and a black residuum which was a pyrophorus when heated; the strong smell of the product occasioned an examination for hydrocyanic acid, but none was found; the small quantity of the matter did not allow of any examination for cyanogen.

## *Conclusions.*

I. The organic matter of the soil of the cave of Kühloch, destructible by fire, is formed of

1st. A fatty acid, which in my examinations presented the properties of stearic or margaric acid. 2dly. A fatty matter which was not acid. 3dly. An organic acid soluble in water. 4thly. A yellow colouring principle. 5thly. A brown azotized matter.

A portion of the yellow colouring principle and of the azotized matter is certainly combined with alumina and peroxide of iron. It is probable that another portion of the organic matters is united with the subphosphates and the subcarbonates of lime and magnesia; it is also probable that in this latter part, there is proportionally more azotized matter than in the former.

There is more organic and pulverulent matter in the specimen taken from a depth of six feet, than in that from a depth of only two feet.

II. There is in the soil some chloride of potassium and ammoniaco-sulphate of potash. Consequently the chloride of potassium and the sulphate of potash arising from the decomposition of the ammoniaco-sulphate of potash by heat, which could not be collected in the process adopted in the analysis of the incinerated soil, must augment the loss occurring in the analysis.

III. The proportions of sulphate of lime indicated (15) and (15') in the ashes of the soil, are not so great as those which really exist in it, because, during the calcination, a portion of sulphuric acid is decomposed.

IV. It is probable that a portion of phosphate of magnesia is combined with phosphate of ammonia.

## ARTICLE VII.

*Additional Experiments and Observations on the Application of Electrical Combinations to the Preservation of the Copper Sheathing of Ships, and to other Purposes.* By Sir Humphry Davy, Bart. Pres. R.S.\*

I HAVE already had the honour of communicating to the Royal Society the results of my first researches on the modes of preventing the chemical action of fluid menstua, such as saline solutions, or sea water containing air, on copper, by the contact of more oxidable metals.

For some months I have been engaged in a series of new experiments on this subject, so important to the navigation and commerce of the country; and through the liberal and enlightened views of Lord Melville, and the Lords of the Admiralty, who desired the Commissioners of the Navy Board and of the Dock Yards to give me every assistance in their power, and all the facilities which our magnificent Naval establishments at Chatham and Portsmouth furnish, I have been enabled to conduct my operations upon a very large scale. At this advanced period of the session, it will be impossible for me to give more than a very short notice of experiments which have been tried under a great variety of circumstances, and the details of which would occupy some hours in reading; but I cannot deprive myself of the pleasure of stating the satisfactory and conclusive nature of the results, many of which have even surpassed my expectations.

Sheets of copper, defended by from  $\frac{1}{4}$  to  $\frac{1}{16}$  part of their surface of zinc, malleable and cast iron, have been exposed, for many weeks, in the flow of the tide in Portsmouth Harbour, and their weights ascertained before and after the experiment. When the metallic protector was from  $\frac{1}{4}$  to  $\frac{1}{16}$ , there was no corrosion nor decay of the copper; with smaller quantities, such as from  $\frac{1}{32}$  to  $\frac{1}{64}$ , the copper underwent a loss of weight, which was greater in proportion as the protector was smaller; and as a proof of the universality of the principle, it was found

\* From the Philosophical Transactions for 1824. Part II.

that even  $\frac{1}{100}$  part of cast iron saved a certain proportion of the copper.

The sheeting of boats and ships, protected by the contact of zinc, cast and malleable iron in different proportions, compared with those of similar boats and sides of ships unprotected, exhibited bright surfaces, whilst the unprotected copper underwent rapid corrosion, becoming first red, then green, and losing a part of its substance in scales.

Fortunately, in the course of these experiments, it has been proved that cast iron, the substance which is cheapest and most easily procured, is likewise most fitted for the protection of the copper. It lasts longer than malleable iron, or zinc; and the plumbaginous substance, which is left by the action of sea water upon it, retains the original form of the iron, and does not impede the electrical action of the remaining metal.

I had anticipated the deposition of alkaline substances in certain cases upon the negatively electrical copper. This has actually happened. Some sheets of copper that have been exposed nearly four months to the action of sea water, defended by from  $\frac{1}{10}$  to  $\frac{1}{20}$  of their surface of zinc and iron, have become coated with a white matter, which, on analysis, has proved to be principally carbonated lime, and carbonate and hydrate of magnesia. The same thing has occurred with two harbour boats, one of which was defended by a band of zinc, the other by a band of iron, equal to about  $\frac{1}{10}$  of the surface of the copper.

These sheets and boats remained perfectly clean for many weeks, as long as the metallic surface of the copper was exposed; but lately, since it has become coated with carbonate of lime and magnesia, weeds have adhered to these coatings, and insects collected on them; but on the sheets of copper, defended by quantities of cast iron and zinc, bearing a proportion below  $\frac{1}{100}$ , the electrical power of the copper being less negative, more neutralised, and nearly in equilibrio with that of the menstruum, no such effect of deposition of alkaline matter or adherence of weeds has taken place, and the surface, though it has undergone a slight degree of solution, has remained perfectly clean: a circumstance of great importance, as it points out the *limits of protection*; and makes the application of a *very small* quantity of the oxidable metal more advantageous in fact than that of a larger one.

The wear of cast iron is not so rapid, but that a mass of two or three inches in thickness will last for some years. At least the consumption in experiments which have been going on for nearly four months does not indicate a higher ratio. This must however depend on the relation of its mass to that of the copper, and upon other circumstances not yet ascertained (such as temperature, the relative saltness of the sea, and perhaps the

rapidity of the motion of the ship :) circumstances in relation to which I am about to make decisive experiments.

Many singular facts have occurred in the course of these researches. I shall mention some of them, that I have confirmed by repeated experiments, and which have connexions with general science.

Weak solutions of salt act strongly upon copper; strong ones, as brine, do not affect it: and the reason seems to be, that they contain little or no atmospheric air, the oxygen of which seems necessary to give the electro-positive principle of change to menstria of this class.

I had anticipated the result of this experiment, and upon the same principle of some others.

Alkaline solutions, for instance, impede or prevent the action of sea water on copper; having in themselves the positive electrical energy, which renders the copper negative. Lime water even, in this way, renders null the power of action of copper on sea water.\*

The tendency of electrical and chemical action being always to produce an equilibrium in the electrical powers, the agency of all combinations formed of metals and fluids is to occasion decompositions, in such an order that alkaline, metallic, and inflammable matters are determined to the negative part of the combination, and chlorine, iodine, oxygen and acid matters to the positive part. I have shown in the Bakerian Lecture for 1806, that this holds good in the Voltaic battery. The same law applies to these feebler combinations. If copper in contact with cast iron be placed in a vessel half full of sea water, and having its surface partially above that of the water, it will become coated with carbonate of lime, carbonate of magnesia, and carbonate of soda; and the carbonate of soda will gradually accumulate till the whole surface in the air is covered with its crystals:—and if the iron is in one vessel, and the copper forming an arc with it in another; and a third vessel of sea water in electrical connexion by asbestos or cotton is intermediate, the water in this intermediate vessel continually becomes less saline; and undoubtedly, by a continuance of the process, might be rendered fresh.

I shall not take up the time of the Society by referring to some obvious practical applications of these researches, to the preservation of finely divided astronomical instruments of brass by iron, of instruments of steel by iron, or zinc: my friend Mr. Pepys has already ingeniously taken advantage of this last circumstance, in inclosing finely cutting instruments in handles or cases lined with zinc, and many other such applications will occur. I cannot conclude, without mentioning particularly my

\* I am at present engaged in applying this principle to experiments on the preservation of animal and vegetable substances.

obligations to Sir Byam Martin, the Comptroller, and Sir Robert Seppings, the Surveyor of the Navy, for the interest they have taken, and the zeal they have shown in promoting these researches; and without stating how much I owe to the care, attention, and accuracy of Mr. Nolloth, Master Ship-wright, and Mr. Goodrich, Mechanist in the Dock-yard at Portsmouth, in superintending the execution of many of the experiments.

### ARTICLE VIII.

*State of the Copper on a Ship's Bottom with Excess of Protection, and of the Cast Iron Protector, after a Voyage to the West Indies.*

WE have received the following communication from Dr. Traill, of Liverpool, who states, that Mr. Horsfall is the owner of the vessel mentioned in his letter, and adopted Sir Humphry Davy's method of defending the copper sheathing of ships' bottoms, immediately on its promulgation. Dr. Traill adds that the surface of the protecting metal to that of the copper somewhat exceeded the proportion assigned by Sir H. Davy, being from  $\frac{1}{100}$  to  $\frac{1}{50}$  of the whole copper surface of the ship.

The first fact mentioned in Mr. Horsfall's letter has been long known. Mr. Daniell formed a similar substance artificially, eight years since, by immersing a cube of grey cast iron in diluted muriatic acid (see *Journal of Science*, vol. ii.); and in the same *Journal* (vol. xii. p. 407), mention is made of a cast iron gun, which by long immersion in sea water was incrustated to the depth of an inch with a substance having all the exterior characters of impure plumbago. As to the state of the Tickler's bottom, it is obviously owing to the copper having been over protected. (See *Annals of Philosophy*, vol. viii. N. S. p. 364.) It is stated in the place referred to, that the requisite proportion of defending surface to that of the copper, as far as had then been ascertained, is somewhere between  $\frac{1}{100}$  and  $\frac{1}{50}$ ; the proportion at present adopted in the Royal Navy, we believe, does not exceed  $\frac{1}{50}$ . In the preceding paper from the *Philosophical Transactions*, our readers will observe that on sheets of copper defended by quantities of cast iron and zinc in a less proportion than  $\frac{1}{50}$  no deposition of alkaline matter or adherence of weeds took place, and the surface, though it had undergone a slight degree of solution, was perfectly clean, "a circumstance of great importance, as it points out the *limits of protection*; and makes the application of a *very small* quantity of the oxidizable metal more advantageous in fact than that of a larger one."

Mr. Horsfall's letter is a candid statement of a fair experiment, and the result, as far as it can be learnt, is the saving of

the whole of the copper. Whether in short voyages the adhesion of small barnacles is a disadvantage which more than compensates this saving, we are ignorant; but if so, a *smaller quantity* of protecting surface must be used; and the exact proportions for different vessels and voyages will be known after a few experiments. Sir Humphry Davy is still earnestly pursuing his inquiries on this important subject; and, we believe, has discovered some new and interesting facts with respect to the conducting powers and electrochemical changes of metals in saline solutions; which enable him to preserve a considerable portion of the copper without any great consumption of oxidable metal. In the experiments now going on, a nail of zinc or iron is, we believe, placed *under* the copper, and in contact with it; and the moist paper upon the wood of the ship in which the nail is placed preserves the electrical circuit with the sea water; so that there is no appearance of protection on the *outside*, though each sheet of copper has its own protector of  $\frac{1}{300}$  or  $\frac{1}{400}$  its surface.

*Extract of a Letter from Charles Horsfall, Esq. to Dr. Traill.*

*Liverpool, Feb. 19, 1824.*

“The Brig Tickler arrived here from Kingston, in Jamaica, about three weeks ago. She had been out on the voyage from this port to Jamaica and back, not quite five months; previously to her sailing she had been new coppered. Bars of cast iron three inches broad, and one inch thick, covering about 100th part of the surface of the copper, were placed upon each side of the keel from the stem to the stern, and fastened on with copper spike-nails. The Tickler went into the Graving Dock to-day. I attended before the water had quite left her; and immediately on the iron on the keel being visible, I went into the dock to examine it. The usual crust of red rust appeared upon it, but on applying a ship’s scraper to it, I found the iron quite soft, to the depth of nearly half an inch. A quantity was scraped off which had all the appearance of black lead, and on handling, it soiled the fingers in the same way that black lead does, and became *quite hot* in the space of a minute or two; the inner part of the iron bar, or that next the copper, being quite hard. I wrapped a small quantity in paper, and put it in my pocket; and on taking it out again, in about a quarter of an hour, it had become very hot, and smoked, and soon assumed the appearance of rusted particles of iron. The bars of iron had been very little reduced in substance during the voyage.

“With respect to the copper, such part of it as was not covered with barnacles appeared bright; and, as far as I could judge from such an inspection of it, as perfect and entire as

when it was put on; but *I never saw a ship's bottom more thickly studded with barnacles*, nor any that were more difficult to strape off. They were all rather small. It was only on the lower part of the bows, and about two inches above, and four inches below the iron bars that the copper was not covered with barnacles; excepting the upper part of it which had been little under water.

"Several vessels are expected to return from the East and West Indies in the ensuing month, having had wrought iron applied in the same manner that the *cast* iron was in the Tickler.

(Signed) CHARLES HORSFALL."

## ARTICLE IX.

### *Astronomical Observations, 1825.*

By Col. Beaufoy, FRS.

*Bushey Heath, near Stanmore.*

Latitude  $51^{\circ} 37' 44.3''$  North. Longitude West in time  $1^{\circ} 20.93''$ .

March 4.	Emersion of Jupiter's first satellite . . . . .	{	9 <sup>h</sup> 07' 55"	Mean Time at Bushey.
			9 09 16	Mean Time at Greenwich.
March 4.	Emersion of Jupiter's second satellite . . . . .	{	11 34 54	Mean Time at Bushey.
			12 36 14	Mean Time at Greenwich.
March 11.	Emersion of Jupiter's first satellite . . . . .	{	11 02 24	Mean Time at Bushey.
			11 03 45	Mean Time at Greenwich.

Observed Transits of the Moon and Moon-culminating Stars over the Middle Wire of the Transit Instrument in Sideral Time.

1825.	Stars.	Transits.
March 3.—	10 Leonis . . . . .	9 <sup>h</sup> 28' 01.37"
3.—	4 Sextant . . . . .	9 41 26.93
3.—	208 Sextant . . . . .	9 46 43.33
3.—	14 Sextant . . . . .	9 47 41.12
3.—	Moon's First or West Limb . . . .	10 0 17.74
3.—	43 Leonis . . . . .	10 13 54.18
3.—	35 Sextant . . . . .	10 31 18.93
4.—	55 Leonis . . . . .	10 46 25.72
4.—	62 Leonis . . . . .	10 54 42.09
4.—	Moon's { First or West Limb . . . .	10 58 45.33
	{ Second or East Limb . . . .	11 01 05.29
4.—	69 Leonis . . . . .	11 04 51.66
4.—	4 Leonis . . . . .	11 07 49.61

## ARTICLE X.

*On a Scarlet Sub-chromate of Lead, and its Application to Painting and Calico Printing.* By John Badams, Esq.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

Feb. 16, 1825.

It was announced by Dulong in the *Ann. de Chim.* so long ago as the year 1812, that by boiling together carbonate of lead and chromate of potash in excess, a red sub-chromate of lead is produced, containing exactly double the quantity of lead in the common chromate. Ten years afterwards, in the same journal, Grouvelle announced the existence of a red chromate, and gave several processes for preparing it, but made no mention of Dulong's discovery. It is remarkable that neither of these chemists, nor any subsequent writer, appears to have noticed the important uses to which this substance is applicable.

Having made a variety of experiments upon its properties, as a fast colour in calico printing, and a durable pigment for artists in oil and water, I wish to give publicity to the advantages which, I think, the arts will derive from its adoption.

Grouvelle's method of preparing red chromate of lead consists in boiling the common yellow chromate with potash; for practical purposes this is much more convenient than Dulong's process, and gives a very fine colour. By some unaccountable blunder, however, the ingenious author of this formula so completely mistakes the nature of the substance produced by it, that every step he takes in his analysis to explain the composition of the red chromate, only tends to involve it in deeper obscurity. But he must speak for himself.

"J'ai analysé comparativement le chromate jaune, le rouge, et le plombrouge de Sibérie. Tous donnent *exactement* le même rapport entre l'*acide* et l'*oxide*. Ce sont des chromates neutres; seulement le chromate rouge contient une petite quantité d'alcali, qui m'a paru être de 1 à 1.5 : " and yet in the very next page, the author contradicts himself, and says, in summing up,

"Il suit de là que l'alcali paraît être combiné à de l'oxide de plomb, et que cette combinaison unie au chromate de plomb, donne lieu au chromate rouge, qui contient ainsi un peu *plus d'oxide* de plomb que le chromate neutre."

Grouvelle then goes on to assert, that he finds a little alkali (lime) in the native red lead of Siberia, though, as he admits, this very ore becomes yellow on being powdered, which, if it really contained lime, it ought not to do.

I shall now endeavour to show that the whole of this analysis is erroneous.

(A.) 100 grains of scarlet chromate of lead made by Grouvelle's process, were digested for half an hour with constant stirring in

very diluted acetic acid. The red colour gradually but totally disappeared, and was succeeded by a pure yellow. This yellow powder, carefully washed and dried at  $300^{\circ}$ , weighed 60 grains, and proved on examination to be common chromate of lead.

Here then we have red chromate of lead (said by Grouvelle to contain exactly the same proportions of oxide and acid as the yellow chromate) resolved into 60 grains of yellow chromate and 40 grains of some other substance, which we shall see, by the next experiment, to be oxide of lead.

(B.) The acetic acid holding in solution 40 grains of the original weight of the red chromate employed, was perfectly colourless and transparent; and, on being gently evaporated in a water bath, shot entirely into crystals, bearing the well-known character of acetate of lead. On redissolving these crystals in water, and boiling them with an excess of carbonate of ammonia, a dense white precipitate fell, weighing, when dried, 46 grains = 38.45 nearly of oxide of lead.\*

(C.) To ascertain whether any sensible portion of potash originally entered into the composition of this red chromate, the ammoniacal solution was evaporated gently in a large platina crucible, and gradually exposed to a red heat: the whole residue sublimed, leaving only a scarcely perceptible stain of lead upon the surface of the crucible. A little water now boiled in the same crucible did not affect the colour of turneric paper in the slightest degree; hence no potash whatever can be suspected.

On repeating these experiments, I feel satisfied that the 40 grains of loss sustained by the red chromate in Experiment (A), were entirely due to oxide of lead; and that 100 grains of red chromate were resolved into 60 grains of yellow chromate and 40 oxide of lead. But 60 grains of yellow chromate are composed of 40.98 oxide of lead, and 19.02 chromic acid; hence it cannot be doubted that yellow chromate contains just half the proportion of oxide in the red chromate, in which case the atomic constitution of the two chromates of lead will be as follows:

Yellow chromate.			Red chromate.		
Chromic acid ..	19.02	1 atom	Chromic acid	19.02	1 atom
Oxide of lead ..	40.98	1 atom	Oxide of lead	81.96	2 atoms
Or 100 parts consist of					
Chromic acid .....	31.7		Chromic acid .....	18.84	
Oxide of lead .....	68.3		Oxide of lead .....	81.16	
	100.00			100.00	

In order further to show the true composition of red sub-

\* The difference between 38.45 and 40 is owing to the imperfect precipitation of oxide of lead by carbonate of ammonia.

chromate of lead, as I may now venture to call it, I took 60 grs. of yellow chromate and ground them with 40 grains of oxide of lead, adding small quantities of hot water from time to time: they united into a red sub-chromate.

#### *Uses.*

I have endeavoured at some length to clear up the theory of the two chromates of lead, because one radical error of the nature which Grouvelle has committed, leads to endless mistakes and disappointments in the practical operations which are founded upon it. I have before alluded to the advantageous use of sub-chromate of lead in giving a permanent orange scarlet upon cotton. The mode of its application to calico printing will be obvious to all who know the principle upon which yellow chromate of lead is made fast; and no intelligent calico printer will need any further information from me, as his own practice in fixing the yellow chromate, added to what has been here said of the nature and preparation of the red sub-chromate will readily suggest what is necessary. I shall only remark that nitrate of lead and an alkaline solution of chromate of potash, will give him the colour. He may accumulate, besides, some insoluble salts of lead in the pores of the cloth to give stability to the tint, and may modify the operation by various means, but in all cases the colours must be heightened at last, by passing them through boiling water.

Scarlet sub-chromate of lead is extremely beautiful when ground up with oil, and possesses great *body* as a pigment. It is not degraded in its hue, like vermilion, by admixture with white lead; it mingles with other colours, and shows no signs, after a long exposure, of any change by time. As a water colour I dare not yet say that it has been tried sufficiently to authorise a positive declaration that it will not blacken, but several pieces of cards and thin paper painted with it, and hung upon the walls of inhabited houses, likely to influence the colour of salts of lead, have not in some months perceptibly diminished in brightness. Should a longer experience confirm the promise already given, I shall be happy to make it known to artists, as they have nothing equal in colour to red lead which they can trust in their drawings for a month even, and no tint would be a more desirable accession to the pallet, than a bright and permanent scarlet, or scarlet orange.

## ARTICLE XI.

*Proceedings of Philosophical Societies.*

## ROYAL SOCIETY.

*March 5.*—The reading of Dr. Williams's paper on the Maternal-fœtal Circulation, was resumed and concluded. This essay gave an account of the different speculations entertained on the nature of the medium circulating between the uterine and umbilical vessels, and considered the evidence brought forward in their support to be unsatisfactory. It then stated, that it had occurred to the author that it might be practicable to arrive at more satisfactory proofs in favour of one or the other of these speculations by observing the phenomena which would present themselves in the fœtal vessels on injecting oil into the maternal vessels, while their irritability was yet active. Experiments were consequently instituted. From their result, Dr. W. is disposed to conclude, that the maternal and fœtal systems in the canine species, are parts only of a common uninterrupted sanguiferous system. From analogy, Dr. W. also infers the communication between parent and fœtus to be similar in all viviparous animals; and remarks, that if his conclusion and inference can be admitted, that we shall have reason to doubt the validity of the doctrine of the maternal-fœtal circulation as taught by Dr. Harvey, together with its modern superstructure. For that if his, Dr. W.'s, experiments and deductions be correct, we can no longer subscribe to the hypotheses of there being two independent sanguiferous circulations in the impregnated state, and to that of the placenta being an organ of respiration or aeration.

Dr. J. R. Johnson, FRS. communicated Some further Observations on the Genus *Planaria*; in which he stated that Mr. Dalyell, of Edinburgh, in a work on the *Planariæ*, having asserted that an individual of *P. cornuta* accidentally wounded near the head produced a new head from the incision, he conceived that the verification of so curious a fact would be interesting to the Royal Society, and accordingly took one hundred of the animals, and made an incision in the side of each; but one of them, however, produced the new head: in the greater number, the wound healed, and in some preternatural excrescences only were produced. Dr. J. proceeded to detail some further remarks on the reproductive faculties of the *Planariæ*, and to describe *P. nigra*, of which a drawing was annexed. It has the abdominal proboscis like the others.

*March 10.*—J. H. Green, Esq. was admitted a Fellow of the Society; and Mr. Herschel communicated a paper entitled Improvements on Leslie's Photometer; by W. Ritchie, AM. Rector of the Academy of Tain.

*March 17.*—The names of Dr. J. L. Tiarks and Dr. J. Richardson were ordered to be inserted in the printed lists of the Society; and the Society for promoting Animal Chemistry communicated a paper by Sir E. Home, Bart. VPRS. entitled Observations on the Influence of the Nerves and Ganglions in producing Animal Heat.

*March 24.*—Major C. Hamilton Smith was admitted a Fellow of the Society; and a paper was read, containing Results of Meteorological Observations taken at the Madras Observatory; by John Goldingham, Esq. FRS. These results are for a period of twenty-six years, extending from 1796 to 1822; and are given in a variety of tables, with explanatory remarks.

In consequence of the approaching fast and festival, the Society then adjourned over two Thursdays, to meet again on April 14.

#### ASTRONOMICAL SOCIETY.

*Feb. 11.*—The fifth Annual General Meeting of the Society was this day held at the Society's rooms in Lincoln's Inn Fields, for the purpose of receiving the Report of the Council upon the state of the Society's affairs, electing Officers for the ensuing year, &c. &c.

The President, H. T. Colebrooke, Esq. in the Chair.

The Report, which was read by Dr. Gregory, and ordered to be printed for distribution amongst the members, commenced by expressing the gratification felt by the Council on witnessing the growing prosperity of the Society, and the increasing evidence of the utility of its institution. It proceeded to state, that for the purpose of still further alleviating the labour of the practical astronomer (the Society having already published in vol. i. part 2, of its Memoirs, tables for facilitating the computation of the apparent places of 46 principal stars), the Council had deemed it desirable that tables of precession, aberration, and nutation, should be computed, embracing, 1st, all stars above the 5th magnitude; 2nd, all stars to the 6th magnitude inclusive, whose declination should not exceed  $30^{\circ}$ ; and 3d, all stars to the 7th magnitude inclusive, within  $10^{\circ}$  of the ecliptic; and that a considerable portion had already been computed under the superintendence of Mr. Baily and Mr. Gompertz, and would be forthwith published, accompanied by an explanatory preface, drawn up, at the request of the Council, by Mr. Baily. The Report then noticed, in terms of well-merited panegyric, the very valuable collection of astronomical tables lately published by Dr. Pearson, the Treasurer; and it will be no little gratification to the scientific world to be informed, that the tables constitute only a part of a comprehensive treatise on Practical Astronomy upon which Dr. Pearson is still engaged. It then adverted to the visit of Mr. Herschel, the Foreign Secretary, to

Italy and Sicily, from which, besides other very considerable benefits, the Society had derived increased facilities of communication with the continental astronomers, nearly the whole of whom the Society had now the honour of numbering amongst its Associates. The Report contained a just tribute of respect to the memory of the late Major-General John Rowley, of the Royal Engineers, FRS. and a member of this Society, of which he was a cordial friend from its commencement. After alluding to the acquired stability and acknowledged utility of the institution, which might justify an application to the Crown for a Charter of Incorporation, the Report stated that the expediency of such an application would most probably engage the consideration of the Council for the ensuing year.\* It concluded by strenuously advising concert and co-operation, observing, that though much had been done to advance astronomical science, and much was in progress, much yet remained to be done. "On the retrospect of the past, however, your Council derive confidence with regard to the future. Let the zeal, activity, and talent of the Members and Associates for the next ten years but keep pace with the efforts of the last five, and the most interesting, brilliant, and beneficial results may unhesitatingly be anticipated."

A list of the papers read at the ordinary meetings, followed by a numerous list of benefactors, and a gratifying statement of the Society's finances, was then read, after which the Members present proceeded to ballot for the Officers for the ensuing year, when the following were declared to have been duly elected :

*President.*—Francis Baily, Esq. FRS. and LS.

*Vice-Presidents.*—Charles Babbage, Esq. MA. FRS. L. and E.; Rev. John Brinkley, DD. FRS. Pres. RIA. and Prof. Ast. Univ. of Dublin; Davies Gilbert, Esq. MP. VPRS. and FLS.; George Earl of Macclesfield, FRS.

*Treasurer.*—Rev. William Pearson, LL.D. FRS.

*Secretaries.*—Olinthus G. Gregory, LL.D. Prof. Math. Roy. Milit. Acad. Woolwich; John Millington, Esq. FLS. Prof. Mech. Phil. Roy. Inst.

*Foreign Secretary.*—J. F. W. Herschel, Esq. MA. FRS. L. and E.

*Council.*—Captain F. Beaufort, RN. FRS.; Major T. Colby, Roy. Eng. LLD. FRS. L. and E.; Henry T. Colebrooke, Esq. FRS. L. and E. and LS.; Bryan Donkin, Esq.; Rev. William Dealtry, BD. FRS.; Benjamin Gompertz, Esq. FRS.; Stephen Groombridge, Esq. FRS.; Edward Riddle, Esq.; Richard Sheepshanks, Esq. MA.; Edward Troughton, Esq. FRS. L. and E.

The Society afterwards dined together at the Freemasons' Tavern, to celebrate their fifth anniversary.

*March 11.*—There was read "An Account of the Arrival and

Erection of Fraunhofer's large Refracting Telescope at the Observatory of the Imperial University at Dorpat :'' communicated in a letter from Prof. Struve to Francis Bailey, Esq. President. Prof. Struve received this telescope in November last, and was happy to find that although it had travelled more than 300 German miles, its several parts had been so carefully packed that none of them had sustained the slightest injury. When in a perpendicular position, the height of the object glass is 16 feet 4 in. (Paris measure) from the floor, 13 feet 7 in. of which belong to the telescope itself; so that the eye-glass stands 2 feet 7 in. from the floor. The diameter of the object-glass is 9 Paris inches (about 9 $\frac{1}{4}$  inches English). The weight of the whole instrument is about 3000 Russian pounds. It is so constructed that it may be used as an equatorial. The upper part of the instrument consists of the tube, with its axis of motion, two graduated circles, and a variety of levers and counterpoises, producing the most perfect equilibrium in every direction, and providing against all friction. The declination circle is divided from 10' to 10', but by means of the Vernier may be read off to 5". The instrument may be turned in declination with the finger, and round the polar axis with still less force.

The most perfect motion round the polar axis is produced by means of clock-work, which is the principal feature of this instrument, and the greatest triumph for the artist, the mechanism being as simple as it is ingenious. A weight, attached to a projection connected with the endless screw, overcomes the friction of the machine. The clock vibrating in a circle regulates the motion, by moving an endless screw connected with a second wheel in the above projection. The weight of the clock as well as that of the friction apparatus may be wound up without the motion being interrupted. When the telescope is thus kept in motion, the star will remain quietly in the centre, even when magnified 700 times. At the same time there is not the least shake or wavering of the tube, and it seems as if we were observing an immoveable sky.

But the artist has done still more; he has introduced a hand on a graduated dial of the clock, by which the motion of the latter can be instantly altered; so that a star may be brought to any point of the field of vision to which it may suit the observer to carry it, accordingly as it is required to make the course of the instrument go faster or slower than the motion of the heavens; and if once placed, it may be kept in that position by returning the hand to its original position. The same mechanism is also used to make the motion of the instrument coincide with that of the Sun and Moon.

This instrument has four eye-glasses. the least of which magnifies 175 times, and the largest 700 times.

M. Struve has compared the power of this telescope with Schroöter's 25-feet reflector, by means of which that astronomer saw  $\sigma$  *Orionis*, twelve or thirteen fold; whereas Struvé clearly ascertained the existence of sixteen distinct stars.

This instrument is furnished with four annular micrometers of Fraunhofer's construction, and an excellent net-micrometer of the same artist. By means of these it appears that the probable error in the measurement of some minute distances of 7" and under, did not exceed the 18th part of a second. The expense of this instrument was about 950*l.* sterling.

There was also read a paper on "A New Zenith Micrometer;" by Charles Babbage, Esq. FRS. &c. The object of the inventor in this instrument is to supersede the necessity of extreme accuracy in the divisions. The principle on which this instrument depends may be readily comprehended by imagining a parallelogram, admitting of free motion about its four angles, to be placed with two of its sides in a horizontal position, and the whole in a vertical plane; and a telescope to be fixed at right angles to the lower horizontal bar of this parallelogram. Here every motion of one of the perpendicular bars of the instrument round its upper joint will not change the angle which the telescope makes with the meridian; but will merely remove it into a new position in which it will point to the same object in the heavens. But if either of the horizontal bars of the instrument be lengthened by a very small quantity, this parallelism of the telescope will no longer be preserved, but any movement of the upright bars round their axes will not only remove the telescope from its position, but will cause it to form a very small angle with its former direction. The magnitude of that angle will depend on the alteration in the length of the arm of the parallelogram, and also on the angle which that arm makes with its first direction. The minutiae of the construction depend upon these considerations, but cannot be rendered intelligible without a diagram. The arc which is actually measured in the heavens by means of this instrument is determined by a formula, in which the sum of three arcs is taken from the semicircumference, one of them resulting from the actual observation; the other two from a cosine and a tangent, ascertainable by computation from the theorem itself. In an extensive use of this micrometer, tables may easily be formed to facilitate the computation.

#### GEOLOGICAL SOCIETY.

*Jan. 21.*—A paper was concluded, entitled "On a recent Formation of Freshwater Rock Marl in Scotland, with Remarks on Shell Marl, and on the Analogy between the ancient and modern Freshwater Formations;" by Charles Lyell, Esq. Sec. GS.

The rock marl described in this communication is an extremely compact limestone, in part of a crystalline structure, and traversed by numerous irregular tubes or cavities.

As a principal part of its geological interest is derived from its recent origin, the author has drawn a brief sketch of the physical structure of the county of Forfar, in order to explain distinctly its position.

Those strata are also enumerated in which limestone is found, and its remarkable scarcity in Forfarshire pointed out.

The districts to which shell marl is confined are next considered, and it appears that deposits of this nature are accumulated only in lakes in two formations, viz. the inferior or transition sandstone, and the old red sandstone.

The Bakie Loch, in which the rock marl occurs, lies in a hollow in sand and gravel. This gravel consists of the broken and rounded masses of the primitive rocks of the Grampians, which are heaped in large quantities upon the old red sandstone in the valley of Strathmore.

The succession of the deposits of sand, shell marl, and rock marl, in the lake of the Bakie now drained, is then described. The shells and plants enclosed in the rock are the same as those in the soft shell marl, and are all still living in the waters on the spot. Among the plants are the stems and seed vessels of *Charæ*, the latter being fossilized in such a manner as to present a perfect analogy to the gyrogonite of the ancient freshwater formations.

Mr. Lyell then considers the probable origin of the rock marl, which appears to be derived from the subjacent shell marl, through which springs ascend, charged with carbonic acid.

Some remarks are next offered on the shell marl of Forfarshire, and some which the author has examined near Romsey, in Hampshire, is described. The subjects of chief interest with regard to the shell marl are, its slow growth, the small proportion of full grown shells, which are found in it in Forfarshire, the greater rapidity of its growth in the vicinity of springs, its abundance in a part of Scotland in which limestone is very rare, and its scarcity in the calcareous districts of England.

The question is then considered whether the shell marl be exclusively derived from the exuviae of testacea, and the various arguments for and against this hypothesis are entered into.

In conclusion Mr. Lyell takes a general view of the analogy between the ancient and modern freshwater formations.

Both of these may be described generally as consisting of thin beds of calcareous, argillaceous, and arenaceous marls, together with strata of sand and clay, to which the consolidated beds bear upon the whole but a small proportion.

The shells and plants contained in both are referable to the same genera.

The bones and skeletons of quadrupeds are found buried at various depths in the marls of Forfarshire as they occur in the lower freshwater formation of Paris.

Of the four desiderata mentioned by Messrs. Cuvier and Brongniart (*Ess. on the Env. of Paris*, p. 56), as being requisite to complete the analogy between the deposits of lakes now existing, and those of a former world; three are supplied by the lakes in Forfarshire, viz. 1. A compact limestone; 2. Vegetables converted into the substance of their calcareous matrix; 3. Large beds of yellowish white calcareous marl.

The rock marl of Forfarshire closely resembles the Travertino of Italy, part of which is a recent formation, but part has been proved by M. Brongniart to be of a date probably as ancient as the upper freshwater strata at Paris.

The only difference remaining between the ancient and the modern freshwater formations is, 1. The absence in the latter of silica, which is only known as a modern deposit from water connected with volcanic agency; and 2. The small scale on which the recent accumulations proceed.

If these differences are ascribable to a higher temperature prevailing where the ancient freshwater rocks were formed, they may perhaps disappear when the hitherto unexplored tropical regions of the globe are fully investigated.

#### MEDICAL SOCIETY OF LONDON.

The fifty-second Anniversary Meeting of this Society was held on Tuesday, March 8, at the London Coffee-house, Ludgate Hill: W. Shearman, MD. President in the Chair.

The Officers and Council for the year ensuing, are:

*President.*—H. Clutterbuck, MD.

*Vice-Presidents.*—H. J. Cholmely, MD.; J. Johnson, MD.; Sir Astley P. Cooper, Bart. FRS.; and W. Kingdom, Esq.

*Treasurer.*—J. Andree, Esq.

*Librarian.*—D. Uwins, MD.

*Secretaries.*—T. J. Pettigrew, Esq. FAS. FLS.; and T. Callaway, Esq.

*Foreign Secretary.*—L. Stewart, MD.

*Council.*—T. Walshman, MD.; W. Shearman, MD.; G. Darling, MD.; T. Cox, MD.; J. Barne, MD.; J. Russell, MD.; J. B. James, MD. FLS.; E. Morton, MD.; G. Drysdale, E. Sutcliffe, B. Brown, J. Dunlap, W. Lake, K. Johnson, S. Ashwell, F. A. Lloyd, J. Handey, E. Leese, H. Edwards, W. D. Cordell, J. Amesbury, W. Burrows, S. Wray, H. B. C. Hillier, M. Gossell, T. W. Chevalier, G. Langstaff, J. C. Taunton, H. Hensleigh, J. M. Mugglestone, J. S. Smith, R. W. Bampfild, R. Brien, R. Blick, M. Ware, Esquires.

*To deliver the Oration in March, 1826.*—J. Haslam, MD.

*Registrar.*—J. Field, Esq.

Mr. E. A. Lloyd delivered the Annual Oration. The subject was, the "Constitutional Treatment of Organic Diseases." The Fellows and their friends then dined together at the London Tavern, Ludgate Hill.

In conformity with the will of the late Dr. Anthony Fothergill, the Society offers the annual gold medal, value 20 guineas, for the best dissertation on a subject proposed by them, for which prize the learned of all countries are invited as candidates.

The subject for this year is "The Nature and Treatment of Carcinoma."

1. Each dissertation must be delivered to the Registrar in the Latin or English language, on or before the 1st of December.

2. With it must be delivered a sealed packet with some motto or device on the outside, and within the author's name and designation, and the same motto or device must be put on the dissertation, that the Society may know how to address the successful candidate.

3. No paper in the handwriting of the author, or with his name affixed, can be received; and if the author of any paper shall either directly or indirectly discover himself to the Committee of Papers, or any member thereof, such paper will be excluded from all competition for the medal.

4. The prize medal will be presented to the successful candidate, or his substitute, at the Anniversary Meeting of the Society in March, 1826.

5. All the dissertations, the successful one excepted, will be returned, if desired, with the sealed packet, unopened.

\* \* \* The subject of the dissertation for the year 1826-7 is "Contagion and Infection."

*Medical Society's House, Bolt-court, Fleet-street,  
March 17, 1825.*

## ARTICLE XII.

### SCIENTIFIC NOTICES.

#### CHEMISTRY.

#### 1. *Condensation of a Mixture of Hydrogen and Oxygen by pulverulent Platinum.*

Döbereiner has ascertained that *moist* as well as *dry* platinum causes the mutual condensation of these two gases. The effect in both cases is equally complete; the only difference being in the length of time necessary to produce it.

The best method of performing the experiment is to ignite at the bottom of a glass tube closed by fusion at one extremity, a quantity of the double ammonio-muriate of platinum, or to decompose in it a solution of platinum by means of a rod of zinc.

In either case, a thin film of platinum is deposited upon the interior of the tube, and adheres with considerable firmness. If a tube thus prepared be filled with a mixture of hydrogen and oxygen (or atmospheric air), and inverted over water, the whole of the hydrogen will be condensed into water in the course of a few hours. A similar result is obtained by placing a mass of spongy platinum well soaked with water into a receiver filled with the mixture of the two gases. He next examined what would be the effect of moistening the platinum with other liquids. With alcohol the experiment succeeded equally as well as with water; but not the slightest condensation took place when the spongy metal was imbibed with nitric acid, or with liquid ammonia. He ascribes these differences exclusively to the gaseous mixture being absorbable by water and alcohol, but not by nitric acid or liquid ammonia: in the former case only, the gases would be conveyed into immediate contact with the metal. Döbereiner concludes with observing, that the existence of some peculiar and independent property in the platinum is more decisively evinced by the present experiment than by any other which he had heretofore made.

These experiments suggested an easy method of depurating hydrogen from minute traces of oxygen. All that is necessary is to enclose it in a stoppered phial, a portion of whose interior has been coated by the process just described, with a thin incrustation of platinum. The oxygen will by degrees undergo condensation.—(Schweigger's *Neues Journal für Chemie und Physik*, xii. 60.)

## MINERALOGY.

### 2. *Sodalite*.

A mineral, obviously intimately associated with sodalite, has been examined by Wachtmeister. It is found on Vesuvius incorporated with the garnet described in p. 71. Its colour is white, and it is in an imperfect degree transparent. It has a granular texture, and is brittle. Before the blowpipe it melts without giving off any water: it is more fusible than albite or icespar, but less so than mesotype or meionite. In borax, it dissolves with extreme slowness into a transparent glass. With solution of cobalt, the edges become faintly blue coloured. Muriatic acid cannot be detected by means of oxide of copper.

The mineral is readily decomposed by nitric or muriatic acid, gelatinous silica remaining undissolved. Its constituents were found to be,

Silica. ....	50.98
Alumina. ....	27.64
Soda. ....	20.96
Muriatic acid. ....	1.29

Wachtmeister considers it a compound of 1 atom of bisilicate of Soda + 2 atoms of silicate of alumina. His results differ materially from the analyses of sodalite which have been made both by Borkowsky and Arfwedson; and on comparing his mineral with the specimen analyzed by the latter chemist, he observed several discrepancies between them, both in their external appearance and in their blowpipe characters. (Kong. Vet. Acad. Hand. 1823, p. 131.)

### 3. Notice respecting the Discovery of a Black Lead Mine in Inverness-shire.

The only mines of black lead which have hitherto been wrought in Scotland are those of Cunnock, in Ayrshire, and of Glenstrathfarrar, in the county of Inverness.\* This last mine was discovered so recently as 1816, but does not seem to have been wrought to any extent.

Under such circumstances, therefore, it is with great satisfaction that we announce to our readers the discovery of another black lead mine in Inverness-shire, on the property of Glengary. The mine is situated near the top of a rocky ravine, close to the head of Loch Lochy, on the south-east side, and within a mile of the Caledonian Canal. The mine is so situated that an artificial trough or slide, of simple construction, like that one used at Alpnack in Switzerland for timber, might be erected to convey the black lead ore by its own force of descent from the mine to the Caledonian Canal.

The breadth of the vein is in many places, where it crops out, fully three feet in breadth.

Not more than a ton or two of ore has been yet taken from the mine, and that too merely gathered from the surface.—(Edinburgh Journal of Science.)

## MISCELLANEOUS.

### 4. New Work on Fossils.

We have the pleasure to announce the appearance of the first century of the *Icones Fossilium Sectiles*, by Charles König, Esq. of the British Museum. This work will be found to possess great interest both for the general naturalist and the geologist, and consists of eight folio lithographic plates, containing exceedingly accurate and well-executed figures of 100 species of fossils, with their descriptions in Latin. Some of the figures are copied from other works, which from their high price or rarity are not within every one's reach: the rest are drawn from nature. The plates are divided by longitudinal and transverse lines into separate compartments, so that the subjects, as the name of the work implies, may be cut out, and arranged in orders and

\* Black lead has been found in Glen-Ely and Shetland.

genera according to the fancy of the purchaser; no systematic arrangement being adopted in the work. The explanations of the figures are short and confined to the descriptions of new genera, the localities of species, and the reasons that have induced the author, in some instances, to adopt new names, who reserves more ample details for a future work, which, from the manner he has executed this, we hope will not be long before it sees the light. The place which each genus occupies in Cuvier's *Regne Animal* is given, and the primary section, class, order, and family, respectively denoted by a period, colon, semicolon, and comma annexed, the tribe being without any mark. Thus the name of the genus *Ixa* is followed by the words (Articulata. Crustacea: decapoda; brachyura, Canceres). We shall only add, that we wish the authors of modern works on natural history would write as elegant Latin as that in which Mr. König has couched the short preface at the beginning of his book.

#### 5. On the Structure of Rice Paper.

The substance commonly known by the name of *rice paper* is brought from China in small pieces, about two inches square, and tinged with various colours. It has been for some time used as an excellent substitute for drawing paper, in the representation of richly coloured insects, and other objects of natural history, and has been employed in this city with still more success in the manufacture of artificial flowers.

Although rice paper has a general resemblance to a substance formed by art, yet a very slight examination of it with the microscope is sufficient to indicate a vegetable organization. In order to observe and trace the nature of its structure, it was necessary to give it some degree of transparency, and I expected to accomplish this by the usual process of immersing it in water or in oil of the same refractive power. This operation, however, instead of increasing the transparency, rendered the film more opaque, and suggested the probability that, like tabasheer, it was filled with air; and that the augmentation of its opacity arose, as in the case of that siliceous concretion, from the partial absorption of the fluid. In order to expel the air from the cells in which it seemed to be lodged, I exposed a piece of the rice paper to the influence of boiling olive oil. The heat immediately drove the air in small bubbles from the cells near the margin; but it was with some difficulty that it was forced to quit the interior parts of the film. As the olive oil had now taken the place of the air, and filled all the cells, the film became perfectly transparent, and displayed its vesicular structure when placed under a powerful microscope.

The rice paper consists of long hexagonal cells, whose length is parallel to the surface of the film; these cells are filled with

air, when the film is in its usual state; and from this circumstance it derives that peculiar softness which renders it so well adapted for the purposes to which it is applied. When the film is exposed to polarised light, the longitudinal septa of the cells depolarise the light like other vegetable membranes.

Among the three specimens of rice paper which I have produced, there is one from which all the air has been expelled by the boiling oil; another in which some of the air bubbles still appear in the vesicles, the air having been only partially expelled by boiling water; and a third, which is in contact with water, without having been deprived of any of its air bubbles.

Upon mentioning to Mr. Neill the preceding experiments, he informed me that the lady in Edinburgh, Miss Jack, who had employed rice paper with such success in the manufacture of artificial flowers, had learned from her brother, who was in China, that it was a membrane of the bread fruit tree, the *artocarpus incisifolia* of naturalists.—(Edinb. Journal of Science.)

## ARTICLE XIII.

### NEW SCIENTIFIC BOOKS.

#### PREPARING FOR PUBLICATION.

Shortly will be published, *A Series of Tables, giving the French Weights and Measures reduced to the English Standard.* By C. K. Sanders, of the Royal Engineers.

• *The whole Works of the late Matthew Baillie, MD. with an Account of his Life.* By James Wardrop, Esq. Surgeon Extraordinary to the King.

*A Narrative of the Source of St. Peter's River, Lake Winnepeck, &c.* By W. H. Keating, AM. 2 vols. 8vo.

*Species Conchyliorum, or Descriptions of all the known Species of Recent Shells.* By G. B. Sowerby, FLS. &c. Illustrated by coloured Plates by J. D. C. Sowerby, FLS. &c.

*The Mine Laws of Mexico, from the original and last enacted Code, are now translating from the Spanish, and, with Observations on Mines and Mining Associations, are nearly ready for the press, under the Editorship of a Barrister.*

#### JUST PUBLISHED.

*A Description of the Faults or Dykes of the Mineral Basin of South Wales.* By G. Overton, Civil Engineer. Part I. 4to. 9s.

*A General Critical Grammar of the English Language, on a System novel and extensive, exhibiting Investigations of the Analogies of Language written and spoken.* By S. Oliver, jun. Esq. 8vo. 12s.

*A Key to the Knowledge of Nature, or an Exposition of the Mechanical, Chemical, and Physical Laws of Matter.* By the Rev. R. Taylor. 18s.

*Analecta Latina Majora, on the Plan of Dalzell's Analecta Græca.* 8vo. 9s. 6d.

## ARTICLE XIV. NEW PATENTS.

E. Lees, Little Thurrock, Essex, publican, for improvements in water-works, and in the mode of conveying water for the purpose of flooding and draining lands; applicable also to other useful purposes.—Feb. 19.

T. Masterman, Dolphin Brewery, Broad-street, Ratcliffe, Middlesex, brewer, for an apparatus for bottling wine, beer, and other liquids, with increased economy and dispatch.—Feb. 19.

E. Lloyd, North End, Fulham, for a new apparatus from which to feed fires with coals and other fuel.—Feb. 19.

B. Tarrow, Great Tower-street, London, ironmonger, for improvements in buildings, calculated to render them less likely to be destroyed or injured by fire than heretofore.—Feb. 19.

J. Ross, Leicester, hosier, for a new apparatus for combining and strengthening wool, cotton, and other fibrous substances.—Feb. 19.

J. Mould, Lincoln's Inn Fields, Middlesex, for improvements in fire-arms.—Feb. 19.

H. Burnett, Arundel-street, Middlesex, for improvements in machinery for a new rotatory or endless lever action.—Feb. 19.

J. Beacham, Paradise-street, Finsbury-square, cabinet-maker, for improvements in water-closets.—Feb. 19.

J. Ayton, Trowse Millgate, Norfolk, miller, for an improvement or spring to be applied to bolting mills for the purpose of facilitating and improving the dressing of flour, and other substances.—Feb. 19.

D. Edwards, King-street, Bloomsbury, writing-desk manufacturer, for an ink-stand, in which, by pressure, the ink is caused to flow to use.—Feb. 26.

J. Manton, Hanover-square, gun-maker, for improvements in fire-arms.—Feb. 26.

W. H. Hill, Woolwich, Lieutenant of Artillery, for improvements in machinery for propelling vessels.—Feb. 26.

G. A. Kallmaun, of the Friary, St. James's Place, Professor of Music, for improvements in the mechanism and construction of pianofortes.—Feb. 26.

J. Heathcoat, Tiverton, lace-manufacturer, for his improved method of producing figures or ornaments on goods manufactured from silk, cotton, &c.—Feb. 26.

J. Bateman, Upper-street, Islington, for a portable life boat.—Feb. 26.

C. Whitehouse, Wednesbury, Stafford, whitesmith, for improvements in manufacturing tubes for gas, and other purposes.—Feb. 26.

T. Attwood, Birmingham, for an improved method of making nibs, or slotts, in copper or other metal cylinders used for printing cottons, &c.—Feb. 26.

D. Gordon, Basinghall-street, London, and W. Bowser, Parsons-street, Wellclose-square, iron-manufacturer, for improvements in writing and plating or coating iron with copper.—Feb. 26.

## ARTICLE XV.

## METEOROLOGICAL TABLE.

1825.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.
		• Max.	Min.	Max.	Min.		
2d Mon.							
Feb. 1	W	30.53	30.26	48	30	—	7
2	S W	30.53	29.68	45	34	—	—
3	W	29.81	29.68	40	28	—	—
4	N W	29.86	29.81	45	25	—	—
5	N W	30.18	29.86	33	29	—	—
6	N W	30.36	30.18	34	28	—	—
7	N W	30.36	30.15	42	38	—	53
8	W	30.49	30.15	45	30	—	—
9	N W	30.60	30.49	45	32	—	—
10	W	30.68	30.60	48	28	—	—
11	W	30.71	30.69	48	26	—	—
12	S W	30.71	30.69	42	28	—	—
13	W	30.72	30.59	38	32	—	—
14	W	30.59	30.35	36	34	—	—
15	S E	30.35	30.20	41	39	.48	—
16	S W	30.22	30.20	45	37	—	—
17	S	30.20	30.14	48	39	—	15
18	E	30.54	30.14	48	43	—	10
19	E	30.44	30.34	52	42	—	—
20	S W	30.60	30.44	52	32	—	—
21	N W	30.60	30.60	48	27	—	—
22	N W	30.60	30.37	45	38	—	—
23	S E	30.40	30.37	42	30	—	—
24	E	30.55	30.40	48	32	—	—
25	N E	30.55	30.43	40	32	—	—
26	N E	30.43	29.99	40	33	—	—
27	S	29.99	29.77	45	29	—	13
28	N W	29.92	29.77	44	30	.40	—
		30.72	29.68	52	25	.88	.98

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

## REMARKS.

*Second Month.*—1. Rainy. 2, 3. Fine. 4. Snowy morning. 5, 6. Fine. 7. Cloudy: rainy night. 8, 9. Fine. 10. Foggy morning: fine day. 11. The same. 12. The same: a clear night. 13. A very thick fog this morning: cleared a little, p. m. 14. Foggy morning: gloomy. 15—17. Cloudy. 18. Rainy. 19. Cloudy. 20. Overcast. 21. Foggy: fine, p. m. 22. Hoar frost: a fine day. 23. The same, 24, 25. Overcast. 26. The same: snow about noon. 27. Rainy morning: gloomy. 28. Fine.

## RESULTS.

Winds: NE, 2; E, 3; SE, 2; S, 2; SW, 4; W, 7; NW, 8.

Barometer: Mean height

For the month.....	30.315 inches.
For the lunar period, ending the 10th.....	30.233
For 14 days, ending the 5th (moon north).....	30.261
For 14 days, ending the 19th (moon south).....	30.417

Thermometer: Mean height

For the month.....	37.946°
For the lunar period.....	37.433
For 30 days, the sun in Aquarius.....	37.133

Evaporation..... 0.88 in.

Rain..... 0.98

And by a second gauge..... 1.02

# ANNALS OR PHILOSOPHY.

MAY, 1825.

## ARTICLE I.

### *Biography of Baron Abraham Nicolaus Edelcrantz.\**

THE life and history of a man eminent for the zeal and success with which he has cultivated and advanced the arts and sciences, although more peculiarly the property of the country which gives him birth, belong nevertheless to his whole brethren of the civilised world. It is, therefore, with no small degree of pleasure that we present our readers with the following account of the life of Edelcrantz, whose estimation in Europe as a man of science, while he lived, was shown by the number of learned societies that chose him one of their body, and whose merits, now that he is no more, it is not less useful than it is agreeable, impartially to scan and study.

A. N. Edelcrantz was born in Abo, on the 28th of July, 1754. While his birth-place was thus situated without the geographical limits of Swden, he is nevertheless most justly claimed as a brother by the Swedes. For his family was of that country, his own life was spent there, his whole exertions were devoted to its service, and reaped for him a rich reward both of emolument and honour. The father of Edelcrantz, Charles Abraham Clewberg, was Professor of Theology in the University of Abo, deriving his family name from Klew and Alunda, in Upland, of which district an ancestor of his had been Judge. The wife of the Professor was Charlotte Agatha Fahlenius, a daughter of Bihsop Fahlenius, by a lady of Italian origin, whose name was Charlotte Teppati.

Young A. N. Clewberg did not long enjoy the advantage of the example and direction of his learned father, whom he lost at the tender age of twelve years. But the powers of a mind, happily gifted with natural ability, to which was joined the invaluable accompaniment of persevering assiduity, had already been

\* From the Transactions of the Royal Academy of Sciences of Stockholm.  
New Series, VOL. IX.

sufficiently developed to supply to the literary orphan all with which a parent's counsel would have endeavoured to imbue it.

He became a student of the college at the age of fourteen, and in four years he attained the honour of having the degree of Magister Philosophiæ conferred upon him in Abo, on the 24th of July 1772.

When a young mind happens to possess a great versatility of talent, like that of Clewberg, and is yet unwedded to any one exclusive pursuit, it not unfrequently happens that the temptations to follow each of many various walks of science or of art are so equal, that the votary lingers long upon the threshold of them all, uncertain which shall be made his choice. Nor do we doubt that from this very cause many men, with the resources of whose minds the world has never become acquainted, have lost the fresh and early vigour of their talents, in skimming the surface of a number of studies without collecting their power to fathom the depths of any one. It very often occurs also that the first of many pursuits, in which it is the student's fortune to attract attention or to gain applause, obtains thereby a place in his early esteem, which determines the course of his future studies; and thus often in the walks of science, as in those of politics or of business, the accident of a moment gives the tone to the events of a life or of an era.

Clewberg had applied himself keenly to fathom the profounder depths of mathematical physics; and it was by a work connected with these investigations that he first fixed the public eye upon his attainments. His work was entitled, "*Dissertatio de Observationibus de Alemberti in disquisitionem Newtonianæ legis Refractionis Klingensjernianam*," Ab. 1772. While this production was thus the first that particularly attracted public attention, his eminence in this line of study had already procured for him the distinction of Teacher (Docens) in Mechanical Philosophy and in Literary History. That he had already given his attention to the latter study, we learn from the disputation which he had previously published and defended: *De causis florecentis et marcescentis reipublicæ Litterariæ*. P. 1 and 2. Ab. 1771, 1772.

At this period of the life of Clewberg, it would seem probable that his whole pursuits were purely academic, and that his ambition did not aspire to any thing beyond the desire of animating and directing the studies of the youth of the University. And accordingly, we find Count Ulr. Scheffer, on 16th Oct. 1778, as Chancellor of the Academy, appointing him to the office of "Assistant in Philosophy, in consequence of his eminent talents and attainments."

The early proofs of talent which he gave had fully entitled him to these honours; and among these, one of the most remarkable was his *Dissertatio de Scriptoribus et fontibus Philosophiæ naturalis*, published in 1776; on the 24th January of which year,

being the King's birth-day, he had also enjoyed the distinction of publicly reading a poem of his own composition to the Academy when solemnizing the occasion. Again in 1778, the recurrence of the same day furnished Clewberg with another theme for his muse, and his "Discourse on the King's Birth-day" was read before the Society *Utile Dulci*, and printed afterwards in the fourth volume of the *Vitterhets-nöjen* (or Collection of Poetical Essays).

It seems probable that the attention of Gustavus III. was first attracted to the young Clewberg by the mode in which his poetic genius thus developed itself; and the monarch's regard was probably afterwards confirmed when Clewberg tuned his lyre to strains of sorrow on the occasion of the death of the Queen Dowager Louisa Ulrica in 1782, in his "Funeral Discourse" on that subject. This appears the more likely, as it was the peculiar pleasure of that monarch to search out from among his subjects, those whose singularly happy mental endowments enabled them to unite to a talent for poetry, a zeal for the literature of Sweden. In the mean time, the young poet and philosopher was still allowed to remain in the Academy, where, however, he had been, in the year 1780, advanced to the situation of Librarian, in the room of Olof. Schalberg. In this office, his Majesty was pleased in 1783 to confer upon him, as a proof of his personal regard, the compliment of having his salary placed on a similar footing with that of the Professors themselves.

The conferring of this privilege was not the only proof of the royal regard which the resolution conveying it contained; for it is there expressly mentioned, that "the honour is owing not more to the recommendation of the Chancellor of the Academy, than to the decisive proofs he had himself given of great acquirements, elegant taste, and superior genius."

The residence of Clewberg this year in Stockholm, and the surprising acquaintance he already displayed with the important yet delicate tactics connected with the management of the public theatres of the capital, induced the king, Gustavus III. to call the highly gifted young man from the country to the metropolis, in which the elegance and literature of the kingdom alike centered. His Majesty's mandate of 23d Sept. 1783, appointing the Librarian Clewberg to be Royal Secretary, stated this office to be conferred upon him as a testimony of the Royal esteem for his literary attainments and useful accomplishments. It was then by no means rare, that the cultivation of letters alone, should lead to situations of the greatest elevation and trust in the State.

The poetical reputation of Clewberg now became rapidly extended and established. This arose in part from his labours for the theatre, among which we may notice his "Epilogue to

the Opera of *Atis*," which was brought forward in 1784, and in part from the publication of several poems in the *Vitterhets-nöjen*; yet perhaps more than either, from his famous "Ode to the Swedes," which was published in 1786. For these efforts he received a recompense of the most gratifying description, in his election upon 19th Oct. 1786, to be one of the eighteen of the Swedish Academy, after the death of C. T. Scheffer, Counsellor of State. Upon the 2d Dec. of the same year, he delivered an "Introductory Discourse," of which his distinguished predecessor formed the theme.

Clewberg's connexion with the King and the capital now became more and more intimate. He was chosen Private Secretary by his Majesty on the 31st May, 1787: he had already been appointed to the care of the Privy Purse; he was moreover second in the management of the *Spektaklerne*, or Public Amusements, and he was named a Member of the General Board of Customs on 17th Oct. 1787.

After this period, the life of Clewberg could no longer be so exclusively devoted to the pursuit of letters or science as it previously had been, but was divided between these and the labours of public office. And this crisis in his history was stamped by a distinguished mark of the Royal regard for his merits, by his elevation to the rank of nobility on the 28th April, 1789. After this event he was introduced to Court on the 9th Nov. of the same year, under the No. 2153, and with the name of Edelcrantz. That these new honours and duties did not induce him to undervalue or abandon the cause of science or literature, we need hardly mention. But it would be an omission not to state that at this time, though as yet he had had no means of accumulating money, yet such was his zeal for the cause of philosophy, that he now presented to the Academy of Abo a collection of books, for which a letter of the *Academie Consistorium* of 25th Sept. 1788, signed by Calenius, Porthan, and many others, conveys their grateful acknowledgments.

During the years 1790 and 1791, the country of Edelcrantz was deprived of his presence; in which time it was not the immediate theatre of his exertions, but he laboured with the same zeal abroad in her service. He was then, at the special request of Gustavus, occupied in a journey through England and France, respecting which unfortunately no authentic details have been yet recovered.

We may fix on the year 1793 as that in which the pecuniary circumstances of Edelcrantz were first established upon a more liberal and certain footing, by his obtaining, as Secretary to the King, 1100 rix dollars from the privy purse, and from the funds of the theatre. On the 1st Nov. of the same year was performed his opera of "*Alcides's Entrance into the World*," which was then newly composed by him. At this time also, the title of

Counsellor of State was bestowed on him. On the 9th Nov. 1794, he obtained a seat and a voice in the Court of Chancery; and on the 24th of the same month, he was made Keeper of the Records of the Royal Order of Gustavus.

It is a distinguishing characteristic of the life of Edelcrantz, that his biographer's direction needs only to be directed in a peculiar manner to the careful exposition of its earlier and initiatory history; for his merits soon develop themselves to an extent, and assume an importance, by which the actions of the individual pervade, and are inseparably blended with, the progress of his country, and the advancement of his age.

In whatever quarter of the world a discovery of magnitude or utility was made, it was the care of Edelcrantz immediately to transfer it to Sweden; and so felicitous were his exertions in this useful career, that he was often able to introduce the invention to his countrymen coupled with a signal improvement of his own. Thus it happened with the mechanical system of the telegraph; a system which, from the method of Chappe, was developed by the Swede into a perfect language of signs. In the year 1794, his investigation on this subject was commenced; and by the month of November, he was able to promulgate his improvements on a method so peculiar, that his telegraph immediately received the name of Edelcrantz's or the Swedish. By the help of ten moveable tables, he succeeded in producing 2024 varieties of figure, each of which could be discerned at the distance of 3½ Swedish miles.\* His treatise on the Telegraph, which was published in 1796, has been translated into many languages, and his invention received a prize medal from the Society of Arts, Agriculture, and Commerce, in London. In the Russian war of 1808, this Telegraph was employed in a long chain of observations, consisting of 43 different stations, between Landsort and Gefle; and a particular corps was placed under his own superintendence, and disciplined by himself in the new system of telegraphic tactics.

In 1797 the scientific merits of Edelcrantz, now sufficiently well known and appreciated, procured for him a seat in the Royal Academy, and in the year immediately following, he was raised to the honour of Preses of that body. When he retired from the situation of Preses, which he did on the same year of his election, he chose for the subject of his discourse, *the uncertainty of our knowledge respecting electricity, and, in particular, respecting its power of penetrating the substance itself of bodies*; an instructive essay, which, we regret to say, he never published. The Essays submitted by him to the criticisms of the Academy were all of them such as had for their main object the application of scientific principle to some purpose of practical utility.

\* A Swedish mile is equivalent to about 6½ English miles.

As examples of these may be mentioned his *Essay on a Steam Gauge*, the purpose of which was to ascertain the elasticity of vapour in steam engines; \* *on an economical Method of heating Apartments*; † *on a Stove for drying Grain*; *on the Bleaching of Linen in Holland*; which last treatise was published in the *Economical Annals of the Royal Academy of Sciences*, 1807, p. 102. The description of an Air-pump, into the system of which Edclcrantz introduced the improvement of employing mercury to act as a piston to rarify the air, was inserted in the *Journals of Nicholson, Delametherie*, and many others printed in foreign countries.

In the year 1800, he was honoured by being appointed one of the Knights of the Order of the Polar Star.

In the following year, once more at the special request of Gustavus, conveyed in a royal mandate given at Arboga on the 18th Dec. 1801, Edclcrantz undertook a scientific mission, of which the purpose was the same with that which we have already mentioned, but in prosecuting which, his route was on this occasion discretionary. He accordingly travelled through Germany, Holland, France, and England. The leading objects of this journey were to procure information respecting the best mode of distilling spirits from grain, especially as it was practised in Scotland; ‡ the most advantageous system of funding the debts of the State; the comparative merits of the foreign processes for the manufacture of iron with those of Sweden; &c. These were the main objects of his expedition; but while his attention was of course chiefly occupied with them, a mind like his found time and opportunity in sundry foreign places to make improvements upon the principle or mechanism of various instruments and apparatus. Thus in Berlin, he invented a new and more perfect construction of Papin's Digester; in Paris, a Tearing-measure (Slitningsmätare); § and there also an improvement upon Argand's lamp; and in England, a safety valve for steam engines, &c.

The conclusion of this journey of Edclcrantz brought home a rich harvest of improvements in art, of discoveries in science, of amelioration in agriculture and manufactures, and of observations containing the seed and embryo of many more inventions

\* Kongl. Vet. Acad. Handl. 1809, p. 128.

† K. V. A. Handl. 1812, p. 24, and 159.

‡ The result of Edclcrantz's investigations on this subject was to introduce the Scotch stills into Sweden. We do not know whether this improvement was or was not in fact carried over in diagrams and descriptions, but certainly the simplest and most effectual mode would have been to take over a few of that numerous body, the practical distillers or smugglers of the hills and glens of Scotland, who have long been famous for the unrivalled excellence of their illicit manufacture.—*Trans.*

§ The purpose of which curious instrument would seem to be to estimate the strength of the principle of cohesion among the fibres of various bodies, as cloth, leather, &c. by marking the amount of force necessary to overcome it, and separate them by tearing.—*Trans.*

in all these branches, so great was the acumen with which he at once discerned a new principle of practical utility, and so excellent the tact with which he saw whether its transference would suit the capabilities of his native country. From England, in an especial manner, he carried off a fund of important observations upon manufactures, and remarks on chemical processes connected with the arts, which are there kept secret, and from seeing which, any thing useful could be extracted or carried away only by a person of the keenest acuteness. The account of this journey, which was communicated to his Majesty, has nevertheless been withheld from the public.\* The agricultural implements alone which he carried home to Sweden have been delineated, and may be seen in the *Annals of the Academy of Agriculture*, for 1813.

In the course of the travels we are just considering, Edelerantz formed many acquaintances with the most learned and illustrious of each country through which he passed; and all these he maintained by a constant intercourse of correspondence till his death. Amongst others, we may mention as those with whom he thus formed connexion in Germany, the celebrated Thaer, Count Podeville, von Soden, &c.; in France, Lacepede, Guyton de Morveau, Prony, Lasterie, François de Neufchateau; in England, Sir John Sinclair, Arthur Young, Sir Humphry Davy, &c.

The general result of this journey appears to have been, in no small degree, to cherish and develope that acquaintance with the important science of political economy, which honourably distinguished Edelerantz, and which gave a character of depth and solidity to all the views he suggested, and to every measure he proposed, at the same time that it secured for them a useful adaptation to the necessities of practical commerce, and a wholesome dislike of all unnecessary shackles and restraints upon the intercourse of trade. The time, however, was not yet arrived when he had it in his power to devote himself exclusively to this favourite pursuit.

There seems to be no more remarkable feature in the character of Edelerantz than the perfect versatility of talent which he possessed, and which enabled him equally to fathom the depths of an abstruse science, or to shine among the first of the votaries of the fine arts, or of the muses. It was his eminence in this latter department which first gained him the public eye and the royal favour, and accordingly we now find that his merits as a man of science had never detached him from these pursuits. In

\* The manuscript of these interesting travels in Germany, Holland, and France alone, occupies a space of 134 closely written folio pages. We may be allowed to hope, that a work like this, calculated to reflect so much credit upon Edelerantz, and to diffuse generally so much important observation, may yet be published among the posthumous writings of the author.

1804 he became Director of the Royal *Spectaclerne*, and he retained this charge until 1810. On the 5th May, 1805, he was appointed Superintendent to the Royal Museum, and perpetual President of the Academy for the Cultivation of the liberal Arts. The zeal with which he laboured for the improvement of this Academy is amply testified by the many Discourses which he composed, as well on other occasions, as also on the festival days held by the Society. And it was owing to his exertions in the Diet of 1809, that the pensions and salaries granted to artists were enlarged, and that twelve appointments (of which one-third, composing a first class, were placed on a handsomer footing than the others) were attached to the State for the support and encouragement of students.

The number and variety of Academies of which he was made a member, mark his varied tastes and pursuits, and the general esteem in which he was held. In 1806, he was chosen an ordinary member in the Mathematical Section of the Royal Academy of the Military Sciences. He had already been elected one of the Academy of Music. In 1808, he became a Member of the Royal Academy of Literary History and Antiquities: on which latter occasion his "Introductory Discourse" is worthy of particular attention.\*

Nor must we here omit to mention a striking proof of the unwearied regard with which Edelerantz examined and weighed all the various interests of the sciences and the arts, in the plan which he submitted to the Royal Academy of Sciences for the establishment of an Institution for Technological Education. This is a subject surely of the most extensive interest, and of the deepest importance; but how few of those who have once themselves overcome the difficulties that obstruct the access to science are able to look back upon them, and deign to study for their removal, that future tyros may no longer labour as their predecessors have done. Both the mode in which he proposed to arrange the system of instruction, and the person whom he recommended as qualified to fill the situation of Instructor, were implicitly chosen by the Academy.

But some societies of which Edelerantz was a member often required a more constant attendance and exertion on his part, than even those which we have enumerated. Thus he had at different periods been member of two Building Committees; Chairman of the Committee for regulating the Mint; and moreover had the superintendence of the payment of the salaries connected with that establishment. He was President of the

\* Besides the Academies and Societies we have just mentioned, Edelerantz was a member of every economical society in the kingdom; he was chosen one of the Society of the Admirers of Natural Philosophy (*Gesellschaft Naturforschender Freunde*) at Berlin in 1802; of the Société d'Emulation, and of the Société d'Agriculture at Paris in 1803; Honorary Member of the Board of Agriculture, and of the Society of Arts, Agriculture, and Commerce, at London; of the Società Italiana at Livorno in 1812, &c.

Committee for the Improvement of the Machinery employed in Manufactures; for the Establishment of a Fund for Civil Pensions; of the Investigation-Committee on the Improvement of the Processes for manufacturing Saltpetre: he was member of the Committee for inquiring into the Composition of Fire-rockets, &c. He had, in addition to all this, been Chairman of the General Insurance Establishment since the year 1805; and to this establishment he gave a new constitution, possessing the double advantage of rendering it more useful and efficient as an Institution, at the same time that its revenue became more lucrative and flourishing; thus combining and mutually advancing interests that had hitherto seemed essentially conflicting.

Practical talents such as his are rare in a man of letters, yet so fully known and relied on was his capacity for conducting the executive department of the state, that in 1808 he received a situation under the government in the office of Chancellor of the Court. As a further mark of favour, on the 24th April of the same year, he was complimented with the distinguished honour of being made Commander of the Royal Order of the Polar Star.

During the revolutions in politics which characterised the whole of this period, it was the constant endeavour of Edelerantz keenly to scrutinize, and fairly to weigh, the merits of every proposal for a change, and next to explain fully the true consequences to which it would lead, and so possess his countrymen with a well-founded opinion of what should be warrantably hazarded to gain these results. This was the motive which actuated him to take the share he did in public business in 1809, at the Diet of which year he was President, as well as at those of 1810, 1812, 1815, 1817, and 1818, in the last three of which he was always a member of the Constitutional Committee.

After the revolution which took place in the government, by which the present King of Sweden, then Crown Prince, was placed at the head of affairs, when Bernadotte considered that the institution of an Academy of Agriculture would materially conduce to the advancement of the various arts connected with the rural economy of the kingdom, Edelerantz received a commission requesting his attendance and advice at its organisation. The precise details of what he then suggested cannot now be ascertained, but the general result of his activity and superintendence was immediately attended with the happiest effects. He became the Director of the Institution immediately on its formation in 1812, and by the principles on which he arranged its system of investigation and research, he was able to communicate to it a power of accurate yet extensive observation and inquiry, such as to make its efficiency as perfect as the country either admitted or required. The proofs of this are abundantly furnished in the Annals of the Royal Academy of Agriculture, and in the Annual Reports and Registers (*Arsberättelser och*

Protocoller), and we have it yet more decisively established by looking at the correspondence which he personally maintained in uninterrupted frequency, with not fewer than 22 Economic Societies; a field of occupation surely sufficiently ample to engage the whole of an ordinary man's attention, but which, as we have already seen, was shared by Edelcrantz with other engagements that alike surprise us by their number, their diversity, and their importance. Yet, perhaps, the strongest proof of the sincerity with which all these pursuits engaged his mind, may be confidently referred to the irrepressible ardour with which he laboured to stimulate the activity of the man of science, to awaken the energy of the philosophical agriculturalist or artizan, and the zealous alacrity which he ever evinced to disseminate new facts, to promulgate discoveries, to abolish prejudices of feeling or of habit, and to infuse life and health into the remotest ramifications of the arts of his country.

On the 24th April, 1813, Edelcrantz was named President of the Royal and National College of Commerce. In this office his talents were admirably fitted to produce the happiest effects upon the most momentous interests of the country. He distinguished himself particularly on the occasion of the discussion relative to the Baltic Company on 1st Nov. 1814; on the important point over which so many prejudices have balefully hung in every country, of permitting the use of foreign vessels for the exportation of Swedish wood, on the 12th May, 1817; on the regulation relative to the use of native shipping in the export of the commodities of the country (*Product-placat*), and its abrogation in favour of the vessels belonging to the Netherlands and North America, on the 30th Aug. 1819, &c. On these great questions of state economy, Edelcrantz always advocated the abolition of unnecessary and ill-judged fetters and restraints upon the freedom of commerce between nation and nation; nor did he hesitate acting on the dictates of conscientious duty, fully and freely to lay before the government and the public, his opinions on these subjects, even when he stood alone, or in a small minority of that Board of Commerce already mentioned, of which he was the official head. The merits of the principles of liberal intercourse which he then advocated, it is not the province of his biographer to enlarge upon; but even those who may choose to question the soundness of the principles on which he acted, must confess that they never were supported by a greater weight of reason, or experience, or practical detail, than when they were urged by Edelcrantz. There are few cases in which the proposal of any change must necessarily awaken more keenly conflicting and opposite interests than those that touch commercial regulations of old standing; but to the honour of Edelcrantz with respect to his conduct even here, his memory has already received justice, and in proportion as prejudices

shall be cleared away, it will become more and more appreciated, and will take yet higher ground in the judgment of the country.

Such were the numerous and strong proofs which Edelcrantz received of the confidence and esteem of the government, and such was the honourable manner in which he always discharged the duties it imposed upon him. On the 9th May, 1815, he received the last public expression of regard from the King in being elevated to the rank of Baron, into which he was introduced on the 27th Nov. 1816, under No. 356.

The many services rendered by Baron Edelcrantz to his country are not to be found so much in separate writings or treatises, as in the actual practice or execution of those plans which it was their object to suggest, and which are embodied in the improvements and in the general system of the country. They were sometimes brought forward by himself as an individual, but not unfrequently their merits embraced interests too extensive and momentous, and spoke too plainly for themselves, to allow government to hesitate a moment in adopting and supporting them as their own. Some account of the greater number of them may, however, be found preserved in the Transactions of various Swedish and foreign learned bodies; and not a few of his proposals and reports have been deposited in the archives of the Court.

We have already noticed the improvements made by him on the organisation of the Telegraph, so great as to procure for the new instrument the name of Edelcrantz's; besides this, the principal mechanical inventions of his are as follow: a Steam Engine of a simpler construction than those formerly employed. This machine was applied to numerous purposes; as in mines to pump off water; to the Crown Distillery in the capital; to promote the operations for excavating Telje's Canal, &c. With a view to a construction of this engine on a plan still more simplified than this, he has left behind him two different ameliorations of structure, of one of which there is now a model. The next invention we shall notice is his new Drying Stove for all Kinds of Grain, which he brought forward in 1812, and which gained at once for him the unanimous approbation of the Royal Academy of Sciences, and of the Academy of Agriculture. It is constructed so as to give the power of correctly regulating the temperature in such a manner that the germinating power of the seed may be preserved unimpaired; while at the same time the heat can, when required, be raised as high as  $194^{\circ}$ , or above that point, so as completely to destroy the weevil. Another signal benefit conferred by Edelcrantz on the manufacturers of Sweden was the introduction among them of a Spinning Machine, extremely similar in principle and utility to the famous English mechanism, the secret of which is guarded by them with so much jealousy.

Among the mechanical apparatus invented by Edelcrantz for the promotion of experiment in mechanical philosophy, besides the improved Air Pump which we have already mentioned, he has left behind him a description of a new construction of Papin's Digester, to which we have briefly adverted in a former part of this narrative, as having been made by him at Berlin. In this new form the lid is fixed more tightly than can be done either by means of a screw or of a leather covering, the elasticity of the vapour is accurately measured, and the whole heat required may be applied by a common spirit lamp. Of the other relics of the practical applications of principle, suggested by his genius, we may mention the account of a curious Static Lamp, in which the oil is placed in equilibrium with a small quantity of mercury;—a piece of mechanism which, operating by the compression and expansion of aerial or gaseous bodies, is able to produce a greater degree of artificial cold than any other method can furnish;—an Areometer, on a more minute scale, and capable of more nice and accurate adjustment than those formerly in use; and a valuable apparatus for the maintenance of a determinate and equal temperature, during the process of chemically investigating a substance under the action of intense heat. Many of these subjects still occupied his mind as he lay on his last sick bed, and it was from it that he dictated some of those his valuable views regarding science and experiment, which form his last bequest, and which ought to preserve long in his country a fond remembrance of him who has left no family behind him to emulate his fame, or enjoy his title.

Edelcrantz died at Stockholm on the 10th of March, 1821. He was never married, and his name must be co-existent with his own individual reputation. But that name is surely made more lasting by the merits of him who adorned it, than it could have been by his having his loss bewailed by the fairest number of an affectionate offspring. A man like him must long survive in the dearest recollections of his countrymen, associated in their minds with those comforts which it was his constant object to cherish and promote, and with those studies and pursuits in which it must be the object of the best among them to emulate him: a foundation for a name, surely not less enviable than it is lasting.

He was a man of delicate constitution, and the age of 67, at which he died, was a period of life fully as advanced as his frame seemed to promise. Temperance and regularity in all his habits, a tranquillity of mind, and a cheerfulness of disposition long preserved to him an uninterrupted period of health which he spent in unwearied activity. The debilitating disease (Hæmaturia) which carried him off, did not make its appearance until the last year of his life. Even within the very arms and embrace of death itself, the mind of Edelcrantz retained its

vigour, and his spirit of research its wonted ardour, and he now bent all his energies calmly to study the dissolution of the body, and the extinction of the vital principle. It is on the veracity of one who was an eye-witness of the melancholy yet interesting spectacle, that it is related that Edelcrantz, with the utmost clearness and precision, watched the ebbing of the tide of life, and measuring its progress, compared with the lapse of the passing moments, foreknew and predicted the crisis of the instant when life should close.—It arrived, and Edelcrantz was no more.

Few men have possessed a capacity for exertion equal to that of Edelcrantz. His information was alike remarkable for its accuracy and for its extent. His judgment was distinguished by solidity and perspicacity,—his zeal in the cause of science and the arts was unbounded; and these qualities gave to him a power of clearly expounding and eloquently enforcing his views, which, whenever he brought forward any of his numerous plans of general benefit and practical utility, was sure to produce in him the most agreeably persuasive powers of oratory.

In his private life his manners were most retired and unobtrusive, yet such as ever commanded respect, and sustained the dignity of his character. His house was ever hospitable, without exhibiting profusion; and his conversation was always easy and sprightly, yet never uninteresting. The company which he gathered round him, without being too rigorously exclusive, was always composed of those only who could fully appreciate and enjoy the intellectual and scientific topics about which he was fond of holding converse. Such a general conversation-party was held by him at least once each week during his residence in the capital. When the weather was fine, he used to make a practice of going from the city or from the cabinet to the tranquillity of his country-seat Skugga, situated in the Royal Deer Park, and which he enjoyed as a gift from the King, where he looked out upon those buildings, plantations, and parks, around him, which had been all planned and designed by himself. In this place, however, the only recreation sought by his active and intelligent mind was a mere change of subject upon which to occupy it, a variety in the kind of employment which was to engage his hours.

Even an imperfect delineation of the character, occupations, habits, and discoveries of Baron A. N. Edelcrantz, is more than the author of this little biography aspires to. His object has been to gather a few detached incidents of the life of Edelcrantz, from which the general utility of his proposals, the elevation of his designs, and the amiableness of his private character, may be felt by the reader better than the writer has been able to portray them: just as the placing before an observer's eye the appearance and dimensions of some of the parts of any well-proportional structure, enables him to rear up and place before

his mind with accuracy that whole which they contribute to form; so the sketches here given may place it within the reader's power to fill up the outline, and to form a somewhat just conception of the penetration, the depth, and the solidity of judgment, of the uncommon versatility of talent, of the richly various pursuits that ever aimed at adding to the happiness of the species, and of the excellent and warm heart that forms the character of Edelcrantz.

To those in whom this short notice of his life shall awaken a desire of more intimate acquaintance with the details of his history, we may recommend the masterly treatises of which he is the theme, and which have been already published concerning him. These are the Discourse over Baron A. N. Edelcrantz, already in the second edition, delivered on the 7th April, 1821, by Gust. Lagerbjelke: the Eloge over President Edelcrantz by J. P. Billberg, in the Transactions of the Royal Academy of the Military Sciences for 1821; and the Discourse (yet unprinted) over Edelcrantz in the Swedish Academy, by C. P. Hagberg, within which society we fondly anticipate that his memory will long remain embalmed in the esteem and gratitude of all who revere virtue, or love their country.

The translator's office here expires: nor will he obtrude any observations of his own upon the reader, before whom he has endeavoured to place some of the merits and interesting life of Edelcrantz. He does not in the least doubt that his exertions in this sphere will be considered as well bestowed by the lovers of science in this country, to whom the plain and simple narrative just closed cannot fail to prove a subject of agreeable and useful meditation. It is his only regret that at this distance of time from the death of Edelcrantz, such a character as that of the illustrious Swede should not yet have found an able pen, to do it the justice it deserves, either in an original treatise, or in a happier translation.

## ARTICLE II.

*Description of an Instrument for ascertaining the Specific Gravity of the Urine in Diabetes and other Diseases.* By W. Prout, MD. FRS.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

April 2, 1825.

As the specific gravity of the urine is a point of considerable importance in many diseases of that secretion, and particularly in diabetic affections, and as the common method of determining this by weighing, &c. is troublesome and tedious, I was induced some time ago to have a small portable hydrometer

constructed for the purpose, of which the following is a summary description.\*

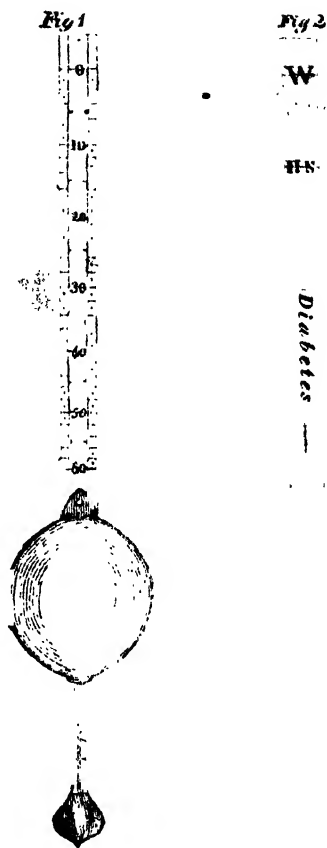
Fig. 1, represents the instrument of its natural size. There is nothing peculiar in its construction but the scale; the numbers on which are always to be added to 1000, the assumed sp. gr. of water. Thus supposing the number cut by the surface of the fluid be 30, this indicates that its spec. grav. is 1030, water being 1000, &c.

Fig. 2, represents the other side of the scale. W (opposite 0 on the other side) is the point at which the instrument stands in pure water. H S or *healthy standard*, is the mean point about which healthy urine usually ranges. The portion of the scale marked *diabetes* is that to which the instrument rises in diabetic affections, &c.

Thus by the aid of this little instrument can every thing connected with the specific gravity of the urine be easily determined in a few seconds to a degree sufficiently accurate for all practical purposes. The scale is graduated for the mean temperature of 60°; but the instrument may be used at all temperatures between 40° and 80° without any error of practical importance. When used, care should be taken to prevent the adhesion of air bubbles, and the scale should be depressed *below* the point at which it naturally stands in the fluid, in order that the instrument may *rise* to that point. The degree then cut (after it has stood a few seconds) by the surface of the fluid *as seen from below* is the specific gravity. When the operation is completed, the instrument is to be dipped into common water, and wiped dry to prevent the corrosion of the metallic part.

I am, Gentlemen, your obedient humble servant,

W. PROUT.



## ARTICLE III.

*A Summary View of the Atomic Theory according to the Hypothesis adopted by M. Berzelius.* By J. G. Children, FRS.

(Continued from p. 193.)

It is obviously necessary for this purpose, that some substance should be fixed upon, the weight of whose atom may be assumed as unity; Dalton chose hydrogen for his unit, as the substance of which the smallest weights enter into combination: he has been followed by Davy, Brande, Henry, Phillips, and various other writers; whilst Wollaston, Thomson, and Berzelius adopt oxygen as their lowest number, that substance being of all others most universally present in inorganic bodies. On the scale of chemical equivalents Dr. Wollaston reckons oxygen as 10, Thomson considers it as 1, and Berzelius as 100. It is of small consequence which atom be selected for the purpose, or what relative value be assigned to it, whether 1, 10, or 100; but whichever be chosen, the weights of the atoms of all other bodies must be expressed in some function of that unit.

The weight of the atom of any body is easily determined, if we know correctly the composition of one or more of the combinations it is capable of forming with any other body, the weight of whose atom has been previously ascertained. Sulphur, for instance, combines with oxygen in several proportions; in the lowest, 100 parts of sulphur take 50 of oxygen; in the next, 100; and in the third, 150;\* numbers which are in the ratio of 1, 2, 3; we may, therefore, assume that in the different oxides an atom of sulphur is united successively to 1, 2, and 3 atoms of oxygen, and the supposition is supported by various considerations of the other combinations of sulphur, as, for instance, those of the sulphurous and sulphuric acids. The lowest compound, therefore, may be considered as containing an atom of each element, and if we call that of oxygen 8, we find by a simple proportion that that of the atom of sulphur is 16.†

This example is sufficient to show the method to be adopted in similar researches, and it is evident that when the weight of the atom of any one body is ascertained, it may be employed for determining that of other bodies.

The results of a mineral analysis may be calculated on the atomic theory, and the inevitable small errors of experiment corrected by its means.

\* There is a fourth compound formed of an atom of sulphurous acid united to an atom of sulphuric acid, and containing 100 sulphur + 125 oxygen. Its atomic composition may be stated as just mentioned, or, as consisting of 2 atoms sulphur + 5 atoms oxygen. It is not necessary to say more about it in this place.

† I adopt the numbers given by Brande and Phillips, in which hydrogen is taken as unity.

Suppose we have found that a sulphuret of lead is composed of

Lead .....	86
Sulphur.....	14
	<hr/> 100

Here a certain number of atoms of lead, whose total weight is 86, were combined with a certain number of atoms of sulphur, whose weight is 14. If, therefore, we divide 86 by the number representing the weight of the atom of lead (which we find in the tables is 104), and 14 by that of the atom of sulphur, (16), suppressing the decimal point in both cases, we find that the compound contains 82 atoms of lead, and 87 atoms of sulphur, numbers which are very nearly equal. Hence we conclude that the mineral is composed of 1 atom of lead and 1 atom of sulphur; and if we calculate the results which our analysis ought to give on this supposition, we find the numbers to be

Lead .....	86·66
Sulphur .....	13·33

which accord very nearly with the results of the experiment.

A similar operation will enable us to find the atomic composition of all other binary compounds, whose analysis is known.

Let us now take an instance of some more complex compounds, and calculate them on the data and numbers assumed by Berzelius.\*

Suppose an analysis of molybdate of lead (a ternary combination) had given,

Oxide of lead .....	61
Molybdic acid .....	39
	<hr/> 100

We find in the annexed table, that the quantity of oxygen in oxide of lead is 7·171 per cent. and that in molybdic acid 33·45; consequently 61 of the former contain 4·37 of oxygen, and 39 of the latter 13·04; but 4·37 : 13·04 :: 1 : 3; or the oxygen of the acid is three times that of the base; but we observe in the tables that the base contains only 2 atoms of oxygen, whilst the acid contains 3; therefore to preserve the ratio of 1 : 3, there must be 2 atoms of acid to 1 of base. The results of the analysis calculated on these data give

Oxide of lead .....	60·86
Molybdic acid .....	39·14
	<hr/> 100·00

\* In which oxygen = 100. The examples are taken from Berzelius, p. 225, et seq. *New Series*, VOL. IX. z

Let us next take an analysis of copper pyrites, and suppose that it has given us

Copper .....	34
Iron. ....	30
Sulphur. ....	36
	<hr/>
	100

The atom of copper by the table is 791.39; that of iron 678.43. Therefore  $\frac{34}{791.39} = 429$  atoms of copper;  $\frac{30}{678.43} = 442$  atoms of iron, and  $\frac{36}{201.16} = 1789$  atoms of sulphur. Now these numbers are nearly as 1, 1, and 4, and consequently the sulphur must be equally divided between the two metals, so as to form bisulphurets, each containing 1 atom of metal, and 2 atoms of sulphur. If we calculate the composition of the pyrites according to these numbers, we shall have

Bisulphuret of copper .....	52.48
Bisulphuret of iron .....	47.52
	<hr/>
	100.00

Or if we take the elements separately,

Copper .....	34.79
Iron .....	29.82
Sulphur .....	35.39
	<hr/>
	100.00

which agrees very nearly with the experimental results, and confirms their accuracy.

Let us now take the analysis of a quaternary compound, a variety of emerald, which gave

	Atoms.
Silica. .... 68.64 or oxygen	34.52 = 8
Alumina .....	17.96      8.38 = 2
Glucina. .... 13.40	4.17 = 1
	<hr/>
	100.00

By the tables, we find the respective quantities of oxygen in the three elements of the mineral as stated above. Now we may consider this compound (says M. Beudant) in two ways, either as consisting of one base (glucina) united to a double acid (silica and alumina), or as a double salt formed of the silicate of alumina and silicate of glucina; both views lead to the same conclusion. In the first case the mineral is supposed to consist of 2 atoms of acid (composed of 4 atoms of silica and 1 atom of alumina) combined with 1 atom of glucina. In the second

manner of considering the compounds, the general law proposed by Berzelius requires that the acid of one of the salts should be a multiple by a whole number of the acid of the other, which may happen in different ways, but in consequence of the tendency of glucina to form salts with excess of acid, the most simple mode is to consider the silica as equally divided between the two bases, which gives us a quadrisilicate of glucina and a bisilicate of alumina. The first of these salts contains 4 atoms of silica and 1 atom of glucina, forming 1 atom of quadrisilicate; the second contains 4 atoms of silica, and 2 atoms of alumina, forming 2 atoms of bisilicate, because all the oxides contain the same number of atoms of oxygen. The composition of the mineral on the first supposition is,

Quadrisilicate of alumina. ....	86.28
Glucina . . . . .	13.72
	100.00

And on the second,

Quadrisilicate of glucina. ....	47.71
Bisilicate of alumina. ....	52.29
	100.00

which are composed of

{ Silica . . . . .	33.99
{ Glucina . . . . .	13.72
{ Silica . . . . .	33.99
{ Alumina . . . . .	18.30
	100.00

Or,

Silica . . . . .	67.98
Glucina . . . . .	13.72
Alumina. . . . .	18.30
	100.00

Let us take another example of a quaternary compound as a good specimen of the mode of reasoning adopted in these calculations.

The analysis of zoisite gives

	Ox.	Atoms.
Alumina . . . . .	33 = 15.41	= 2
Silica . . . . .	43 = 21.62	= 3
Lime . . . . .	24 = 6.74	= 1
	100	

In this case the silica must be so divided between the two bases as to form a silicate of alumina containing 2 atoms of  
z 2

silica and 2 atoms of alumina, and a silicate of lime, in which the quantity of the oxygen in the acid is equal to that in the oxide; the oxygen in the first salt is, therefore, double the oxygen in the second. Now lime contains 2 atoms of oxygen, and silica 3 atoms; consequently to preserve the equality of oxygen in the two bodies, there must be 3 atoms of lime and 2 atoms of silica. The total quantity of oxygen in this silicate is, therefore, 12, and that in the silicate of alumina 24; but in that compound there are only 6 atoms of oxygen; therefore the salt must contain 4 atoms of silicate of alumina. According to this, we have

Silicate of alumina.....	59.47
Silicate of lime .....	40.53
	100.00

which are composed as follows :

Alumina.....	30.66
Silica.....	28.81
Lime.....	26.13
Silica.....	14.40

Or,

Alumina.....	30.66
Lime.....	26.13
Silica.....	43.21

Hence if the substance operated on was pure, a small portion of lime has, in the analysis, been confounded with the alumina.

As another example, and one well worthy to follow the preceding, we will take the analysis of a variety of topaz. The results gave

	Oxygen.	Atoms.
Alumina.....	59	= 27.55 = 5
Silica.....	34	= 17.10 = 3
Fluoric acid.....	7	= 5.09 = 1
	100	

We may consider this mineral either as a compound formed by the combination of a double acid (silica and fluoric acid) with alumina; or as a double salt, consisting of one base united to two different acids; that is, as a fluate, and a silicate of alumina. In this instance, the alumina naturally divides itself into two portions, whose quantities of oxygen are 3 and 2. The first portion is combined with a quantity of silica, containing 3 atoms of oxygen, and forms a silicate; the second is combined with a quantity of fluoric acid, whose oxygen is 1. Hence it follows that the oxygen of the first salt is to the oxygen of the second in the ratio of 6 : 3, or 2 : 1. Now fluoric acid contains 2 atoms of oxygen, and alumina 3; the bi-aluminous fluate must,

therefore, be formed of 4 atoms of alumina and 3 atoms of acid, in order to preserve the ratio of 2 : 1 ; the oxygen of this salt, therefore, is 18 ; but an atom of silicate of alumina contains only 6 atoms of oxygen, because these two oxides have each 3 atoms ; and as the oxygen of the silicate must be double that of the fluate, there must be in this compound 6 atoms of silicate. On these data topaz is formed of

Silicate of alumina .....	68·70
Bi-aluminous fluate .....	31·30
	100·00

Or, taking the elements separately,

Silica .....	33·00
Alumina .....	35·72
{ Fluoric acid .....	7·61
{ Alumina .....	23·67
	100·00

Or,

Silica .....	33·00
Fluoric acid .....	7·61
Alumina .....	59·39
	100·00

Or, on the first hypothesis,

Fluo-silicic acid .....	40·61
Alumina .....	59·39
	100·00

In his *Nouveau Système Minéralogique*, Berzelius frequently calculates the results of the analyses of minerals consisting of metallic alloys, or sulphurets, from the quantity of oxygen which each ingredient would take if reduced to proportionate degrees of oxidation.

An ore of antimoniated silver, analyzed by Klaproth, gave

Silver .....	77
Antimony .....	23
	100

and its atomic constitution is thus calculated by Berzelius,\*

Argent	77	prenant oxygene in degrés	5·798.	2.	77
Antimoine	23	proportionels d'oxidation.	2·850.	1.	23

\* *Nouveau Système*, p. 50.

Now the weights of the atoms of silver and antimony in Berzelius's table are,

Silver. ....	= 2703
Antimony. ....	= 1613

and the protoxide of silver contains 2 atoms of oxygen, and that of antimony 3, and no lower states of oxidation of either metal are mentioned :

$$\begin{array}{l} 2703 : 200 :: 77 : 5.69 \\ 1613 : 300 :: 23 : 4.27 \end{array}$$

But 5.69 : 4.27 is not in the ratio of 2 : 1 ; to obtain which, and reduce the metals to a *proportionate* degree of oxidation, recourse is had to an imaginary oxide of antimony, at a lower degree of oxidation than the lowest known oxide of that metal ; and it is consequently assumed that the 23 parts of antimony, if reduced to the state of oxide, would require only two-thirds as much oxygen as by the tables they ought to take. Is this good logic ?

We have now to exhibit the method invented by the same philosopher for denoting the composition of chemical compounds by symbols, "in order to facilitate the expression of the proportions of their elements, and to enable us to state briefly and easily the number of elementary atoms which any of them may contain." The nomenclature is wholly taken from the Latin.

1. Simple bodies not metallic are denoted merely by the initial letter of the Latin name of each substance, even though the same letter be common to some of the metals ; thus S = sulphur, C = carbonicum, P = phosphorus, B = boracium, (boron), &c.

2. A metal whose initial letter is not common to any other elementary body is denoted, like the preceding substances, by that letter alone, as U = uranium, K = kalium (potassium), L = lithium, &c. ; but if the initial be common to another metal, or to either of the simple non-metallic substances, then the two first letters are taken as Si = silicum, Au = aurum, &c. ; but if both the first and second letters be common to more than one metal, then, instead of the *second* letter of the name, the *first different consonant* is annexed to the initial letter. Thus St = stibium, Sn = stannum, &c.

The chemical sign only denotes a single atom ; if it be necessary to express more than one atom, a figure is placed to the left of the sign ; thus, Cu + O, denotes oxidulous copper, and Cu + 2 O, oxide of copper ; but as this method would be inconveniently long for expressing the composition of a compound of the second order, Berzelius abridges it by omitting the letter O, and denoting the atoms of oxygen by dots placed over the sign of the base, the number of dots indicating that of the

atoms of oxygen combined with it. The oxides of copper, for instance, instead of the preceding signs, are indicated by  $\ddot{\text{Cu}}$ , and  $\ddot{\text{Cu}}$ ; the sign of sulphurous acid is  $\ddot{\text{S}}$ , that of sulphuric acid  $\ddot{\text{S}}$ , and so on; and in the salts of copper  $\ddot{\text{Cu}} \ddot{\text{S}}$ , = oxidulous sulphate of copper, and  $\ddot{\text{Cu}} \ddot{\text{S}}$ , sulphate of copper; the little figure placed above, like an algebraical exponent, indicating that in the latter compound there are 2 atoms of sulphur or sulphuric acid to 1 of base.

The composition of atoms of the third order is denoted after the same manner; for instance,  $\ddot{\text{Ca}} \ddot{\text{C}} + \ddot{\text{Mg}} \ddot{\text{C}}$  represents the mineral called *dolomite*, which is composed of an atom of carbonate of lime, and an atom of carbonate of magnesia. The

formula for alum is  $\ddot{\text{K}} \ddot{\text{S}} + 2 \ddot{\text{Al}} \ddot{\text{S}} + 48 \text{Aq}$ ; and indicates its composition to be 1 atom of sulphate of potash + 2 atoms of sulphate of alumina + 48 atoms of water (aqua). The small exponential figure refers only to the initial sign immediately preceding it; but the coefficient applies to each element contained between the sign +; as, for instance, in the preceding examples, the exponent 1 means that 3 atoms of sulphuric acid combine with the atom of alumina to form the sulphate, and the coefficient denotes that 2 atoms of that salt are taken.

So far the symbols are tolerably simple and intelligible; but we frequently meet with such expressions as the following:—

$\ddot{\text{Al}} \ddot{\text{Si}}$ , silicias aluminicus;  $\ddot{\text{Al}}^2 \ddot{\text{Si}}$ , silicias bialuminicus;  $\ddot{\text{Ca}} \ddot{\text{Si}}$ , silicias, calcicus;  $\ddot{\text{Ca}} \ddot{\text{Si}}^2$ , bisilicias calcicus;  $\ddot{\text{Al}} \ddot{\text{S}}$ , sulphas aluminicus;  $\ddot{\text{Al}} \ddot{\text{S}}$ , sulphas trialuminicus;  $\ddot{\text{Ca}} \ddot{\text{S}}$ , sulphas calcicus, &c. which require a little further explanation.

In order to understand the meaning of these and similar formulæ, it is necessary to state some peculiarities in Berzelius's views with respect to the composition of certain bases.

If we refer to Thomson's or Phillips's table of the weights of atoms, we shall find that lime, baryta, strontita, alumina, magnesia, &c. as well as most of the protoxides of the common metals, as lead, iron, tin, mercury, &c. &c. contain respectively 1 atom of base and 1 atom of oxygen. But Berzelius considers them as containing 2, and some of them 3 atoms of oxygen, for the following reason:—"If we take," he observes, "a comprehensive, general view of the compound bodies that have been correctly analysed, we find that many of them, particularly the oxides, contain decidedly more than two atoms, and that it most frequently is the electro-negative element which enters in a

greater proportion than that of a single atom; soda, oxide of lead, carbonic acid, sulphuric acid, &c. may be quoted as familiar instances. This is still more observable in the combinations of compound atoms, as in the salts, where several atoms of the electro-negative oxide are commonly found united to a single atom of the electro-positive. On the other hand there is every reason to believe that the atoms are only united one to one in those bodies which manifest weak affinities, as the gaseous oxide of carbon, the oxides of copper, mercury, gold, &c.; so that we may presume that all bodies composed of an atom of base and an atom of oxygen have more or less the characters of sub-oxides. It appears, moreover, certain, that the atoms of the stronger acids and bases contain more than one atom of oxygen. Since the number of simple atoms in a compound atom must necessarily influence the form, and consequently the properties of the latter, we have a right to suppose that oxides which contain the same number of atoms of oxygen, have at least some general properties in common which distinguish them from those that have either a greater or a less number. Thus, as we have reason to presume that the oxides which contain only 1 atom of oxygen have the weakest affinities, we find a whole series of more strongly marked salifiable bases, in which the number of the atoms of oxygen must be twice as great as in the former, and therefore it is probable that all the stronger bases contain two atoms of oxygen. Those which contain three atoms of oxygen, on the contrary, are weaker, and many of them may even be electro-negative with respect to some of the electro-positive oxides; \* that is, act as acids.

Amongst the stronger bases, Berzelius includes all the alkalis and alkaline earths, the protoxides of lead, silver, cadmium, iron, copper, tin, zinc, &c. but alumina, silica, and some others, as may be seen in the table at the end of this abstract, he considers as containing 3 atoms of oxygen. Now, having laid down this arbitrary law, two consequences follow, which the student must keep in mind, or he will be liable to fall into error when he endeavours to translate the formulæ into common language, or compares Berzelius's atomic weights with those of English authors. The first is, that to preserve the proportion between the weight of the oxygen and that of the base, as found by analysis, Berzelius has been obliged to double the weights of the atoms of all those elementary substances whose protoxides he considers as containing two atoms of oxygen, and to treble those which contain three. Thus the protoxide of lead, according to the table (see *oxidum plumbicum*), contains per cent. 92·83 lead, and 7·171 oxygen, and  $7·171 : 92·83 :: 100† : 1294·5$ ; but

\* Essai, p. 114, et seq.

† The weight of an atom of oxygen.

the weight of the atom of lead in the table is just double that number, viz. 2589. Again the composition per cent. of alumina is aluminum 53·3, oxygen 46·7, and 46·7 : 53·3 :: 100 : 114·11; which is just one-third of the tabular weight of the atom of

aluminum, viz. 342·33.\* Hence the expression  $\ddot{\text{C}} \ddot{\text{S}}^2$  means the neutral sulphate of lime, and not the bisulphate, which at first view we should probably suppose it to represent, for as the lime contains two hypothetical atoms of oxygen, the salt must also contain two hypothetical atoms of acid, or the canon would be violated which requires that the oxygen of the acid should be a multiple by a whole number of the oxygen of the base. Hence when we find such a symbol as the preceding, and wish to read it correctly, we must remember that all the atoms are doubled, and consequently represented by numbers, which, to reduce them to those of English authors, must be divided by 2. In like

manner in the expression  $\ddot{\text{Al}} \ddot{\text{S}}$  (sulphate of alumina), all the atoms are trebled. As three to one, according to Berzelius's views, represents a neutral salt, so an *equal* number of atoms of acid and base represents a salt *with excess of base*; and accord-

ingly we have  $\ddot{\text{Al}} \ddot{\text{S}}$ , signifying sulphas trialuminicus.

We have not yet quite done with these *simple* views; another

formula remains to be noticed, namely, that of  $\ddot{\text{Al}} \ddot{\text{Si}}$ . After what we have just shown respecting the sulphas trialuminicus, the reader will probably suppose that this expression means silicias trialuminicus. No such thing; it is the neutral compound, silicias aluminicus. But with respect to this inconsistency, hear Berzelius's own confession.

"I must here point out a little inconsistency which I have committed in the nomenclature of the silicates, by applying the term *silicias* to combinations in which the oxygen of the base is equal to that of the silica. The analogy of silica with acids containing 3 atoms of oxygen would require that this appellation should be given to those compounds in which the oxygen of the silica is three times that of the base. It is evident that these are the true neutral silicates, and that the first are salts with excess of base, since the alkalies, by decomposing a silicate with the assistance of heat, always reduce it to that point at which the silica and the base contain equal quantities of oxygen. However, as the study of the silicates belongs principally to that branch of chemistry which is chiefly applied to mineralogy, and as the nomenclature of the numerous degrees of saturation of silica

\* To reduce Berzelius's numbers to Thomson's, divide by 100; and to reduce them to Brande's, multiply by  $\frac{1}{100}$ . Berzelius's number for alumina accords pretty nearly with Thomson's, but not with Brande's or Phillips's. I believe the former to be correct.—C.

becomes much more easy by this method, I have thought it right to adopt it."\*

Now really if the symbols are intended "to facilitate the expression of the proportions of the elements of chemical compounds, and to enable us to state briefly and *easily* the number of elementary atoms which any of them may contain," we can hardly conceive any contrivance less calculated to answer its purpose! For here in the very same table we have two expressions of precisely the same kind, denoting two very different sorts of compounds,  $\ddot{\text{Al}} \ddot{\text{Si}}$ , representing the neutral silicate of alumina, and  $\ddot{\text{Al}} \ddot{\text{S}}$  a subsulphate of the same base.

Again  $\ddot{\text{Ca}} \ddot{\text{Si}}^2$  denotes one neutral salt, silicate of lime, and  $\ddot{\text{Ca}} \ddot{\text{S}}^2$  another equally neutral compound, sulphate of lime.

The formula  $\ddot{\text{Ca}} \ddot{\text{Si}}^2$  represents, as we have just stated, the silicate of lime, and  $\ddot{\text{Ca}} \ddot{\text{Si}}^4$  the bisilicate; the acid of the first containing the same number of atoms as the base, and that of the second twice as many, so that these *brief* and *easy* statements require the reader to multiply the dots over each letter by their respective exponents, and then compare the ratios of the products before he can tell whether the salt be neutral, super-acid, or with excess of base. This indeed is easy enough in the two instances just mentioned, but it will probably require some reflection before the reader perceives the meaning of  $\ddot{\text{Fe}}^3 \ddot{\text{S}}^4 + 6 \ddot{\text{Fe}} \ddot{\text{S}}^2 + 72 \text{Aq}$ , and finds out that it means *sulphas biferrroso-ferricus cum aqua*.

The symbols of organic atoms are denoted, like the inorganic, by the respective initial letters of the Latin names of the substances, but are distinguished from the latter by a line drawn above the letter. Thus  $\overline{\text{A}}$  = acetic acid,  $\overline{\text{C}}$  = citric acid,  $\overline{\text{T}}$  = tartaric acid, &c. So much for the symbols. That they are on the whole ingeniously contrived we do not mean to deny, but that they are necessary or useful we are much disposed to question. At all events they should be consistent with themselves, and the anomaly which we have pointed out between the sulphates and silicates, should, as it very easily might, be done away. If a cypher be required, the key should be constant, and apply equally to every part of it; but there are in our opinion many and serious objections to the adoption of symbols at all. In the first place it requires great care to write them

\* Essai sur la Theorie des Proportions Chimiques, p. 170.

correctly, for a single error may wholly pervert the meaning of a formula, and the consequence of such an error is the more serious, because it cannot, as in common language, be readily detected and corrected by the context. The errors of the press too are more likely to escape notice, and thus this species of danger, from inaccuracy or inattention, becomes doubled. Much habit is required both in writing and reading the symbols, as well as considerable application to become so familiar with them as instantly to comprehend their meaning, especially of the more complicated formulæ; and after all, what is the great benefit they are supposed to confer? A brief and easy method of stating the exact composition of all chemical compounds. For the brevity, it is more than counterbalanced by the risk of error; for the facility, it requires considerable study to learn to do that in one way which every body knows how to do in another without any study at all. But it expresses the exact composition of every substance in all its minutiae—the number of atoms of bases and acids, of the electro-positive and electro-negative elements—and they are too complicated to be expressed in common language without a tedious multiplication of words. They do indeed express the exact composition assigned to the various compounds by the hypothesis of Berzelius; but may not all those compounds be reduced to much simpler forms, and consequently the necessity for expressing them by this short-hand character be done away with? We shall try this question most fairly, by comparing the results of two or three analyses calculated on Berzelius's system, and on the more simple views adopted in this country; and for this purpose we may take some of those copied from Beudant in the preceding pages, and first that of a variety of emerald (see p. 338). The weight of an atom of

Silica.....	= 16
Alumina. ....	= 17*
Glucina . ....	= 26

Proceeding on the principles already explained, we obtain the following quotients by dividing the quantity of each substance, as found by the analysis, by its proper atom.

$$\begin{array}{rcl}
 & \text{Atoms.} & \\
 \frac{6864}{16} & = 429 = 8 \text{ of silica.} & \\
 \frac{1796}{17} & = 105 = 2 \text{ of alumina.} & \\
 \frac{1340}{26} & = 51 = 1 \text{ of glucina.} &
 \end{array}$$

\* The weights of the atoms are from Phillips's table except that of alumina, which we take from Berzelius for the reasons given in the note (p. 345).

The sum of all the atoms = 188\*, and by the rule of simple proportion,† we find the theoretical composition of the mineral to be per cent.

Silica . . . . .	68·09
Alumina. . . . .	18·09
Glucina . . . . .	13·82
	100·00

Analysis of zoisite (p. 339). The atom of lime = 28.

Atoms.	
$\frac{4300}{16} = 268 = 3$ of silica.	
$\frac{3300}{17} = 194 = 2$ of alumina.	
$\frac{2400}{28} = 85 = 1$ of lime.	

The sum of the atoms therefore = 110, and the theoretical composition of zoisite is,

Silica . . . . .	43·63
Alumina. . . . .	30·90
Lime . . . . .	25·45
	99·98

It is needless to multiply instances, as any analyses compared in the same way must obviously give the same results.

Thus we see that all the complicated statements, and still more complicated reasonings, on which they are founded, of which we have given examples in the preceding pages, may be just as well expressed with much greater simplicity, and that the simple statements equally furnish us with a test of the accuracy of our analyses. Our after reasonings as to the mode in which the elements are severally united in the actual mineral, however probable, can only be conjectural, and we are just as likely to form a correct estimate on the simplest as on the most elaborate system. What good purpose then do these complicated statements answer? Do they teach us more accurately the true constitution of mineral substances, or the mode in which their elements are combined? We cannot perceive how. They do indeed, as we have said of the formulæ, give a detailed view

$$\begin{array}{rcl}
 * & 16 \times 8 & = 128 \\
 & 17 \times 2 & = 34 \\
 & 28 & = 26 \\
 & \hline
 & & 188
 \end{array}$$

† 198 : 128 : 100 : x, and x = 68·09, and so with the other atoms.

of their author's hypothesis, but do they therefore prove its accuracy? It seems to us to be reasoning in a circle. The formulæ are made for the hypothesis, and the hypothesis supports the formulæ; but what arguments can be deduced from both together to render it more probable that alumina and silica contain 3 atoms of oxygen, and lime, baryta, &c. 2, than that each of those substances is composed of 1 atom of base and 1 of oxygen? In point of fact, both views come to the same thing; for if we assume lime to contain 2 atoms of oxygen, the weight of the atom of the base (as stated above) must necessarily be doubled, so that in reality whether we represent sulphate of lime

by the formula  $\dot{\text{C}}\ddot{\text{S}}$ , or  $\ddot{\text{Ca}}\ddot{\text{S}}^2$ , we equally express a triple ratio of the oxygen of the acid to that of the base, and so in all other

cases; for  $\dot{\text{Al}}\ddot{\text{S}} = \ddot{\text{Al}}\ddot{\text{S}}^1$ . The adoption of the latter formula, therefore, is as if one should expect to approach nearer to the truth of a proportion by writing 999 : 666, instead of 3 : 2.

But perhaps it may be argued that the hypothesis presents a correct view of the analogies subsisting between all oxidated bases, and enables us to arrange them in separate orders according to certain characteristic properties by which the oxides of one order may be distinguished from those of another. The observations of M. Mitscherlich seem to demonstrate that such distinct orders actually exist, and as the subject is both curious and important, and because we would not willingly suppress any argument that may appear favourable to the hypothesis, we shall dwell a little upon it, although this paper has already exceeded the limits we had originally prescribed for it.

We have another motive also for doing so. We know that one of the first crystallographers of the present day\* thinks favourably of M. Mitscherlich's theory, and our respect for his opinion would alone induce us to treat it with attention. It would give us great pleasure if that gentleman would take up the subject, and correct any errors that either ourselves or others may have fallen into concerning it.

M. Mitscherlich observed that certain bases, saturated with the same acid to the same degree, affect the same crystalline forms, and that lime, magnesia, and the protoxides of iron and manganese compose in this manner one class of what he has called *isomorphous* bases; whilst alumina and the peroxides of iron and manganese form another. He showed also that isomorphous salts have the property of crystallizing together, concurring in an uniform manner in the formation of one and the same crystal. M. Mitscherlich supposed that the primary forms presented by isomorphous bases are really identical, and that this identity necessarily results from a similarity in their atomic constitution,

that is, in the proportions of oxygen contained in the elements of the isomorphous crystals; and that wherever this atomic similarity exists, identity of crystalline form will always be the result. Thus he says the oxygen in the phosphorous and arsenious acids is to that in the phosphoric and arsenic acids as 3 : 5. In the biphosphate and binarsenate of potash, the oxygen of the base is to that of the acids as 1 : 5, and to that of the water of crystallization as 1 : 2.

Hence the only difference between these salts consists in the radicle of the acid of one of them being phosphorus, and that of the other arsenic; and all the salts, which differ only in this manner, are said to present identical crystalline forms.

Berzelius has made considerable use of Mitscherlich's hypothesis to bring together as one species all the varieties of garnet, as well as those of amphibole, mica, and several other minerals; and in vol. ix. New Series, p. 70, of the *Annals of Philosophy*, our readers will find an abstract from Wachtmeister's paper, in the Swedish Transactions, containing a description and analysis of 13 varieties of garnet, all of which, with only one exception, proved to be constituted of an atom of a silicate of a base containing 3 atoms of oxygen, as alumina and peroxide of iron, combined with an atom of a silicate of a base containing 2 atoms of oxygen, as lime, magnesia, protoxide of iron, and protoxide of manganese.

M. Beudant has the following remarks on the same subject. After observing that it is scarcely possible to obtain artificial salts in a state of purity by crystallization from a liquid holding several salts in solution, unless they differ very materially in point of solubility, in which case they crystallize in succession, one after the other, he says, "if on the contrary they are nearly equally soluble, they all mix together in greater or less proportion, and not one of them will be pure. These mixtures often happen indifferently with every species of salts, so that they appear to be the mere effect of chance, and in that case the extraneous portion is always in very small quantity. But mixtures occur under certain circumstances which it is very important to understand, and may then take place in all sorts of proportions, wherefore sometimes no particular ingredient sensibly predominates. In general it is observed that salts of the same order of composition unite most readily, especially when they have nearly similar crystalline forms. Thus all the species of alum have such a tendency to mix together that it is extremely difficult to counteract it, and they cannot be completely separated when once united, even by repeated crystallizations. Mixtures of the same kind occur between nitrate of baryta and nitrate of lead; between the nitrates of potassa and soda; and the sulphates of iron, cobalt, nickel, &c.; also between the sulphates of zinc, soda, and magnesia, &c. &c. These mixtures

not only occur when a solution contains merely the salts of the above-mentioned groups, but if a great number of salts be dissolved in the same liquid, they will form by preference, so that it may be said that salts belonging to the same order of composition, seek each other, as it were, to crystallize together, and mix in every proportion.

"In the case of the mixture of different salts\* of the same formula, it is observed that the crystalline forms are not sensibly affected, for such salts have, if not identical forms, at least forms of the same kind, and very nearly allied with respect to their angles; as was first observed by M. Mitscherlich. Hence we can imagine that at the moment of their becoming solid, a certain number of the molecules of one salt may be substituted for those of another without occasioning any irregularity in the crystallization. This identity of formulae is not only observed between salts with the same acid, and having different bases of the same degree of oxidation, but also between salts of the same base, or bases of similar degrees of oxidation, that have different acids of the same order of composition. Whence it results that not only salts of different bases have analogous forms, more or less nearly allied, but also that salts having different acids are similarly circumstanced."\*

"Mixtures of substances belonging to the same formula of composition are also extremely frequent in nature both in simple and multiple compounds; but as we cannot in this case, any more than in that of artificial salts, separate at will the immediate principles of these bodies, it is only by the consideration of their analyses that we can arrive at a knowledge of those mixtures. Now by this consideration, we find in the simple compounds, that such or such an oxide is replaced by such or such another belonging to the same order of composition. For instance, in stones accidentally coloured by a combined oxide, we find that the colouring principle is some oxide which replaces either that which serves as base, or that which plays the part of an acid. Thus in the silicates with base of lime, or the bioxide of calcium, the colouring matter is frequently the bioxide of iron, and its quantity is such, that its oxygen is precisely equal to that of the lime that is wanting. It follows that the sum of the oxygen of the lime, plus that of the bioxide of iron, is exactly equal to the quantity of oxygen, which the lime would contain in the pure colourless silicate. In silicates with base of alumina, or the trioxide of aluminium, the colouring matters are the trioxide of iron, the trioxide of manganese, &c. sometimes both; and their quantity is such that their oxygen is equal to that of the deficient alumina.

"In multiple compounds, one or other of the immediate prin-

\* *Traité Elementaire*, &c. p. 244.

ciples is often replaced by one or more principles of the same formula, whose quantity varies indefinitely in different analyses, but is always such that its oxygen is equal to that of the principle replaced: hence if we take on the one hand the oxygen contained in the common acid, and on the other the sum of the quantities of oxygen contained in the bases, we obtain numbers which are precisely in the same ratio to each other that they would be if the compound were perfectly pure. We are even led to the knowledge of cases in which one of the immediate principles is replaced by another with a totally different acid, the base either remaining the same, or being itself different. Thus silicates of lime are replaced by aluminates of the same base, or by aluminates of bioxide of iron, &c.\*

M. Beudant then goes on to illustrate the preceding observations by examples, and gives a sort of receipt for making garnets, or rather endeavours to show how one compounded of many elements may be divided into several others of more simple composition.

We shall quote, with some abridgment, his first example.

"There are garnets obviously of the following composition:

Silica . . . . .	41	containing oxygen	20.60	or 2 atoms
Alumina . . . .	22		10.27	1
Lime . . . . .	37		10.39	1
	<hr/>			
	100			

"which indicates 2 atoms of silicate of alumina, plus 1 atom of silicate of lime; and a series of direct analyses presents a multitude of other results that can only be explained by calculating them on the atomic system. Thus the connexion between the following analysis and the preceding is by no means obvious.

Silica . . . . .	37.00	containing oxygen	18.61
Alumina . . . . .	13.50		6.30
Lime . . . . .	29.00		8.14
Magnesia . . . . .	6.50		2.51
Trioxide of iron . . . . .	7.50		2.30
Trioxide of manganese	4.75		1.41
	<hr/>		
	98.25		

"If we collect the oxygen of the bases of the same order, namely, the alumina, trioxide of iron, and trioxide of manganese, on the one hand, and that of the lime and magnesia on the other, we find that the quantities of oxygen in the acid and bases are not far from the ratio of 2, 1 and 1, consequently the new garnet very nearly harmonises with the former.

“ Further to illustrate our analysis, let us insulate each of the species of garnet contained in the mixture. If we employ the trioxide of iron, to make a melanite garnet (*en faisant un grenat mélanite*) of the formula  $2 \text{Fe}^{\text{III}} \text{Si} + \text{Ca}^{\text{I}} \text{Si}$ , we must take a quantity of

Trioxide of iron, whose oxygen = 2.3	corresponds to	7.50
Silica. ....	4.6	9.14
Lime. ....	2.3	8.19
		<hr/> 24.83

“ There remains a portion of lime whose oxygen is 5.84, with which we may make a grossular garnet (*dont on peut faire un grenat grossulaire*) of the formula  $2 \text{Al}^{\text{III}} \text{Si} + \text{Ca}^{\text{I}} \text{Si}$ , by taking a quantity of

Silica, whose oxygen = 11.68	corresponds to	23.22
Alumina .....	5.84	12.50
Lime. ....	5.84	20.79
		<hr/> 56.51

“ There then remains a quantity of silica whose oxygen is 2.33, and to use it up (*pour l'employer*), we may first make a garnet of alumina and magnesia (*on peut faire d'abord un grenat d'alumine et magnésie*) of the formula  $2 \text{Al}^{\text{III}} \text{Si} + \text{Mg}^{\text{II}} \text{Si}$ , by taking a quantity of

Silica, whose oxygen = 0.92	corresponds to	1.83
Alumina .....	0.46	0.98
Magnesia. ....	0.46	1.19
		<hr/> 4.00

“ Lastly, we shall make of the remainder (*on fera de restera*) garnet of manganese and magnesia, of the formula  $\text{Mn}^{\text{IV}} \text{Si} + \text{Mg}^{\text{II}} \text{Si}$ , by taking the residual

Silica, whose oxygen = 1.41	corresponds to	2.80
Trioxide of manganese. ....	0.705	2.38
Magnesia. ....	0.705	1.82
		<hr/> 7.00

“ All these products subtracted, there only remains

Trioxide of manganese .....	2.38
Magnesia. ....	3.47
	<hr/> 5.85

"which may be regarded as merely in the state of mixture;" and of which M. Beudant cannot *make* any thing further. He continues,

"Thus we see that the garnet in question contains

Melanite garnet .....	24.83
Calcareous garnet. ....	56.51
Aluminous and magnesian garnet &c. . .	4.00
Magnesian and magnesian garnet ..	7.00
Trioxide of manganese (mixed) .....	2.38
Magnesia (mixed) .....	3.47
	98.19

This is taking a peep into Nature's workshop with a vengeance, and it is really a pity that all the elements of the analysis could not be worked up; quite provoking that Nature should have employed nearly six per cent. of matter in *her* way of making a garnet, more than M. Beudant wanted for *his*, and still more so that all the elements should be in exact definite proportion in the first compound, and not in the last, so ingeniously dished up from the several ingredients of melanite, grossular, &c. &c. into this garnet olio! Other similar examples are given from the analyses of axinite and amphibole, but the reader will probably think the preceding quite sufficient.

M. Beudant concludes the chapter by observing, that the above method of discussing the analyses of minerals is the only way to form a clear idea of their composition—every other mode of looking at them, he says, "leads merely to vague ideas, or rather leads to nothing at all. The common plan of giving the weights of the insulated ingredients generally presents only a parcel of incoherences, and it is this bad method that has so long prevented the immediate application of chemical researches to mineralogy, by concealing all the advantages that may be derived from them." We strongly suspect we shall adhere to the *bad method*, notwithstanding.

With respect to the term *isomorphous*, M. Beudant very properly remarks, that it cannot be received in a rigorous sense, and that it frequently merely indicates a very strong analogy, the forms of substances, said to be isomorphous, differing only very slightly in the measurements of their corresponding angles.

The late M. Haüy was not a convert to the new views adopted by MM. Mitscherlich and Berzelius. After stating their ideas respecting pyroxene, he says,\* "they were not led to these conclusions by direct observations on the different silicates contained in the pyroxenes, but deduced them from observations made by M. Mitscherlich on different substances obtained sepa-

\* *Traité de Minéralogie*, Second Edition, p. 39.

rately by chemical processes, and compounds of different bases combined with the same acid." M. Mitscherlich has quoted three crystallized substances found in nature as analogous to those he obtained artificially, namely, the sulphates of lead, baryta and strontita. "These analogous compounds," observes M. Haüy, "of three bases combined with the same acid should have the same primitive form, and M. Mitscherlich without doubt has examined closely into the matter to satisfy himself if this example be favourable to his views. The fact is obviously otherwise. The primitive form of sulphate of lead is a rectangular octohedron, and consequently incompatible with that of sulphate of baryta, and sulphate of strontita, which is a right rhomboidal prism. Moreover the angles and dimensions of this prism differ obviously in the two species, the angles of the base in the sulphate of baryta being  $101^{\circ} 32'$  and  $78^{\circ} 28'$ , and in sulphate of strontita  $104^{\circ} 28'$  and  $75^{\circ} 12'$ .

"M. Mitscherlich has not been more fortunate in the identity of form which he fancies he has discovered between two other natural substances, whose composition has nothing in common, namely, sulphate of copper and axinite. The three angles which measure the incidences of the faces of the parallelopipedons, the primitive form of those two substances, are, for the sulphate of copper, the first  $124^{\circ} 2'$ ; the second  $128^{\circ} 37'$ ; and the third  $109^{\circ} 32'$ ; whilst for axinite two are right angles, and the third is  $101^{\circ} 30'$ . Such are the contrasts which M. Mitscherlich takes for characters of identity."

The form of the crystals of sulphate of magnesia and sulphate of zinc is, according to Haüy, a right prism with a square base, terminated very commonly by a right quadrangular pyramid. M. Mitscherlich quotes them as "another example, but the angle," says Haüy, "formed by two of the faces of the pyramid taken on two opposite sides is about  $10^{\circ}$  greater in the sulphate of magnesia than in the sulphate of zinc.

"Moreover, how is it that the results announced by M. Mitscherlich are, on every side, in contradiction to those presented by natural productions, as if affinity played a different part in his laboratory to that which it acts in the laboratory of nature?

"Take a view of the various crystals that are found in our cabinets containing different bases united to the same acid, and throughout their geometrical forms will be seen to differ more or less. The primitive form of phosphate of lime is a regular hexahedral prism, that of phosphate of lead a rhomboid, that of phosphate of iron an oblique rectangular prism, that of phosphate of copper a rectangular octohedron, and that of phosphate of manganese a rectangular parallelopipedon. If we take the muriates, the primitive form of muriate of ammonia is a regular octohedron, that of silver a rectangular parallelopipedon, that of

iron a right rhomboidal prism, and that of copper a rectangular octohedron; and so of the rest."

It does not appear that M. Häuy has taken into consideration the difference that water, chemically combined, may produce in crystalline forms; at least he says nothing about it in his arguments against isomorphism in the passages we have quoted. He continues thus:—"According to these observations, if the opinion of MM. Berzelius and Mitscherlich with regard to pyroxene, a natural mineral, be correct, it follows that its constitution is an exception to the general results of the crystallization of natural bodies, and appears to be inexplicable.

"I must add, that on the preceding idea, it would be very difficult to form a clear idea of what constitutes the species, pyroxene, in a chemical point of view. The different silicates, which occur as constituent parts of that mineral have nothing fixed, either in respect to their number in the same individual, nor in their proportions. Supposing all the combinations of which they are capable, taken one and one, two and two, three and three, to exist in nature, we shall have fifteen different modifications of pyroxene; and if we reflect that in the analyses hitherto made of different pyroxenes, the quantity of magnesia varies from 4.5 per cent. to 30, that of iron from 1.08 to 17.38, and that of manganese from 0.09 to 3, what a series of shades shall we obtain if we multiply those analyses!"

According to Häuy's views, all the pyroxenes contain a common basis of elementary molecules, which determines their true composition, and by a necessary consequence the invariable form of their integrant molecule, and all the other ingredients, which he considers as purely accidental, are only interposed amongst the molecules of the essential substance without affecting its characteristic form. That substance he assumes to be silicate of lime, for in fourteen analyses the quantity of lime was nearly constant, and in the proportion of about 20 per cent. on the whole mass. "I do not know," says he, "why M. Berzelius has supposed that it may be replaced by magnesia; how can it yield a place to that substance which it has never abandoned?"

More lately Mr. Brooke has also questioned the stability of this hypothesis,\* and has asserted (as we have seen that Häuy had done before), that the supposed identity of isomorphous bases does not exist, and that the *apparently* similar forms belonging to substances which differ in composition, do *really* differ from each other in measurement, although in some cases by only so small a quantity as not to be appreciable by the goniometer. Mr. Brooke remarks, that "the instances which M. Mitscherlich has adduced in support of his theory, or we

\* Edinburgh Philosophical Journal, vol. xii. p. 18.

may almost say as its foundation, are not in accordance with it," and he then goes on to show the differences in the inclinations of the planes, in the sulphates of lead, baryta and strontita. "These," he adds, "are natural crystals, and evidently do not support our author's theory."

Mr. Brooke then states, that the artificial salts of those three bases accord with it no better, and that the *acetates* present even much greater discordances than the sulphates. "The theory is not better supported by the carbonates of lime, iron, and zinc, which are stated to be isomorphous. The primary forms of these substances are rhomboids, and the inclination of P on P' has been ascertained to be as follows :—

Carbonate of lime . . . . .	105°	5'
iron . . . . .	107	00
zinc . . . . .	107	40

Mr. Brooke adds, that he is informed that the theory on more mature consideration has been abandoned by the author himself. If that be so, his candour reflects the highest honour on M. Mitscherlich, whilst the necessity of relinquishing a favourite hypothesis furnishes an additional argument against the adoption of those dogmas which, in some measure at least, led to its original formation. For, if Mr. Brooke's information be correct, we must object to them, not merely their negative quality of uselessness, but their positively mischievous tendency to induce or confirm error. But, to return to our original subject, however that may be, the assumption that the stronger bases must contain more than one atom of oxygen, should be established on a better foundation than mere analogy, or such arguments as we have met with in the preceding pages, before it is made the groundwork for superseding the beautiful simplicity of the atomic theory as promulgated by Dalton, and substituting in its stead the unnecessary intricacies introduced by Berzelius.

We are not, however, surprised that the hypothesis should have made considerable progress amongst our fellow chemists on the Continent. Its ingenious promulgator has, we believe, a large number of pupils, and it is perfectly natural that bred in his school they should warmly support and propagate the doctrines of so admirable a master; for in most respects, few chemists in Europe deserve that epithet more justly than Berzelius. The accuracy of his analyses, the incomparable ingenuity which many of them demonstrate, the indefatigable ardour with which he pursues his darling science, and the multitude of important facts with which his genius and industry have enriched it, give him a high claim to the admiration of every chemist in the world. It does not follow, however, that he is therefore infallible, and after the best attention we have been able to bestow on his peculiar modifications of the atomic theory, we see no reason for

preferring them to the simpler doctrines taught in England, and until they shall be fully convinced of their superiority by *facts* derived from *experiment*, we hope the great masters of our own schools will adhere to their present system, both in their lectures and their publications.

(An abstract of Berzelius's table of atomic weights in our next)

## ARTICLE IV.

*Astronomical Observations, 1825*

By Col. Beaufoy, FRS.

*Bushey Heath, near Stanmore.*

Latitude  $51^{\circ} 37' 44.3''$  North. Longitude West in time  $1^{\text{h}} 20.93''$ .

March 17.	Emersion of Jupiter's third satellite . . . . .	{	7 <sup>h</sup> 56' 33"	Mean Time at Bushey.
March 18.	Emersion of Jupiter's first satellite . . . . .	{	7 57 54	Mean Time at Greenwich.
March 18.	Emersion of Jupiter's first satellite . . . . .	{	12 57 10	Mean Time at Bushey.
March 18.	Emersion of Jupiter's first satellite . . . . .	{	12 58 31	Mean Time at Greenwich.
March 20.	Emersion of Jupiter's first satellite . . . . .	{	7 25 50	Mean Time at Bushey.
March 20.	Emersion of Jupiter's first satellite . . . . .	{	7 27 11	Mean Time at Greenwich.
March 24.	Immersion of Jupiter's third satellite . . . . .	{	8 22 29	Mean Time at Bushey.
March 24.	Immersion of Jupiter's third satellite . . . . .	{	8 23 50	Mean Time at Greenwich.
March 27.	Emersion of Jupiter's first satellite . . . . .	{	9 20 32	Mean Time at Bushey.
March 27.	Emersion of Jupiter's first satellite . . . . .	{	9 21 53	Mean Time at Greenwich.
April 3.	Emersion of Jupiter's first satellite . . . . .	{	11 15 27	Mean Time at Bushey.
April 3.	Emersion of Jupiter's first satellite . . . . .	{	11 16 48	Mean Time at Greenwich.
April 12.	Emersion of Jupiter's first satellite . . . . .	{	7 39 25	Mean Time at Bushey.
April 12.	Emersion of Jupiter's first satellite . . . . .	{	7 40 46	Mean Time at Greenwich.

Occultation by the Moon.

March 26. Immersion of a small star. . . . . 9 14 38 Sidereal Time.

Observed Transits of the Moon and Moon-culminating Stars over the Middle Wire of the Transit Instrument in Sidereal Time.

1825.	Stars.	Transit.
April 1.—	57 Leonis . . . . .	10 <sup>h</sup> 47' 15.63''
1.—	61 Leonis . . . . .	11 04 51.64
1.—	φ Leonis . . . . .	11 07 49.74
1.—	77 Leonis . . . . .	11 19 01.04
1.—	ε Leonis . . . . .	11 21 26.10
1.—	Moon's First or West Limb . . . . .	11 25 26.49
1.—	126 Virginis . . . . .	11 29 31.30
1.—	167 Virginis . . . . .	11 42 09.10
1.—	213 Virginis . . . . .	11 52 08.11
1.—	230 Virginis . . . . .	11 57 06.02
2.—	17 Virginis . . . . .	12 05 22.16
2.—	14 Virginis . . . . .	12 10 24.34
2.—	63 Virginis . . . . .	12 14 14.17
2.—	Moon's First or West Limb . . . . .	12 23 56.01
2.—	κ Virginis . . . . .	12 30 17.37
2.—	196 Virginis . . . . .	12 42 21.53
2.—	ν Virginis . . . . .	12 45 20.07

## ARTICLE V.

*On Light and Heat from Terrestrial Sources, and on the Theory of the Connexion between Light and Heat.* By Baden Powell, M.A. FRS.

(1.) In all investigations on radiant heat, one of the principal sources of difficulty consists in properly estimating the loss of heat by radiation from the bulb of the thermometer on the side not exposed to the radiant influence, and which depends on the rate of communication of heat through the bulb, and on the radiating power of its surface. External circumstances regulate the amount of this effect; the proximity of a glass screen of lower temperature increases it as we have already had occasion to notice; and independently of radiation, there must be a trifling loss by conduction to the air in contact; but in all these cases, it is evident that the loss will be very different, according to whether we are observing the rise of the thermometer in a given short time, in a longer time, or its stationary indication. The communication of heat through the bulb will also be very different in a mercurial and in an air thermometer: in the latter also the expansion of the glass will be likely to produce considerable error from the lower conducting power of the inclosed air.

All these circumstances, and perhaps others, have a great tendency to perplex the experimental results; and I have been the more induced here to allude to them, because I am inclined to think that I have not given some of my former arguments the advantage they might have had from attributing too great an influence to the loss by radiation. This probably need not have been taken into consideration in the formula, since it would seem that a greater length of time would be necessary in order to the communication of heat through the bulb so as to produce any sensible loss of heat. It would be easy to investigate a more general and correct formula; but upon reconsidering those experiments (28) to which the formula applies, I am by no means sure whether they are of a nature sufficiently susceptible of precision to determine with any exactness the proportion maintained between the heating and illuminating intensity of the rays. In fact, until we possess that important desideratum, a photometer upon the principle of illumination, this part of the subject must remain involved in considerable uncertainty.

(2.) The consideration above adverted to will apply to the experiments on the solar heat (8), and the remark upon them (42). The conclusion is in fact thus very much strengthened; and the effect of simple heat, if any were added by the removal of the screen, would be to diminish the ratio of the white to the black effect, by addition of quantities to its terms in the ratio of

$\frac{\text{white}}{\text{black}} = \frac{100}{89}$ . These experiments may be contrasted with those recently communicated to the Royal Society, in which precisely the same method was applied to terrestrial light and heat, and a remarkable difference in ratio exhibited when the screen was removed.

Similar remarks apply to the other experiments (19, 20, 42), which appear to me to afford the most satisfactory means, and perhaps the most delicate, we at present possess, of deciding the question as to the existence of any perceptible portion of simple radiant heat in the solar rays.

The difficulties alluded to I have found to occasion much perplexity in the experiments on terrestrial light and heat in which I have been for a long time engaged. In those results which form an answer to the principal question existing on the subject, and which are contained in the paper just alluded to, I conceive all fallacy arising from these causes is sufficiently guarded against; and I trust the same may be said of some further investigations on the same topics, to which I alluded at the end of my last paper; and which were at first designed to form a second part to the paper communicated to the Royal Society; but which upon further consideration I withdrew.\*

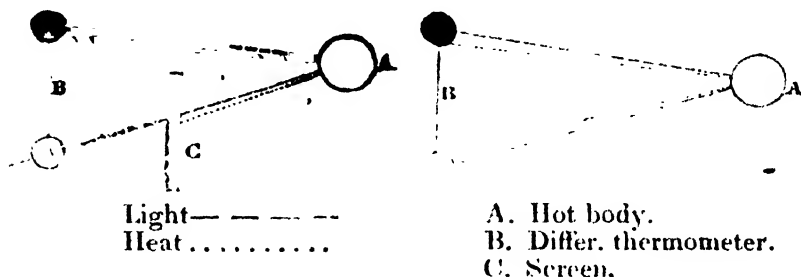
The principal part of these investigations, and the theory which I have deduced from them, together with some additional remarks, will form the subject of the present paper.

(3.) Having by the former experiments, as I conceive, established the general fact of two heating radiations emanating from luminous hot bodies, it becomes obvious that we may apply this distinction to explain many results of former experimenters; in particular those of M. de la Roche, before alluded to, will, upon this principle, exhibit an increase in the ratio of the heating power of light to the simple heat in proportion as bodies are more completely luminous. Wishing, however, to examine this and other kindred phenomena upon a uniform principle, I adopted the following application of the differential thermometer, which, though it will not prove the existence of two radiations, enables us, when their distinct existence is assumed, to determine the ratio subsisting between their effect, though not with great accuracy, yet probably sufficiently so for the purpose here wanted.

The method consists in placing a small screen so as to intercept the heat going to the plain bulb. The black bulb is then affected by the sum of the two radiations, or  $l + h$ . Then observing without the screen in the usual way we have  $(l)$ , and thus obtain  $(h)$  and the ratio  $\left(\frac{l}{h}\right)$ . I here suppose the bulbs to

\* I mention this because, owing to an accidental mistake, some account of them was given in the report in the *Annals* for March, p. 221.

be both alike absorptive of simple radiant heat. This was not exactly the case in the following experiments; one bulb being coated with Indian ink, so that if the radiation were not sufficient to counterbalance the effect, the value given to (*l*) is too great when the instrument was used without its case; but the difference was probably very trifling as will appear by a comparison made in some of the experiments. The annexed sketch will, perhaps, assist in showing the nature of the effects: it requires no explanation.



This is precisely the same method as I formerly adopted for endeavouring to detect any sensible degree of non-transmissible heat in the sun's rays.

(5.) In the instance of the sun then, the heating power of light constitutes the total effect. In the instance of luminous terrestrial sources, we recognise the joint action of the two radiations; and in non-luminous hot bodies only that of heat. In different instances of luminous bodies, these two causes operate in different proportions so as in some to approach the first, and in others the last of these descriptions; and if so, what are the distinctive circumstances with which such variation is accompanied?

(6.) In addition to the inference before made from De la Roche's experiments, it seems well established that (*ceteris paribus*) the light emitted from flame increases with the completeness of the combustion. Thus Count Rumford (*Phil. Essays*, i. 304) found that with equal quantities of oil, the light of an argand lamp was to that of a common lamp as 100 to 85. I was desirous of comparing such a ratio with the corresponding one of the effects of simple heat; and the following are a few results obtained by the method just described, with the flame of an argand lamp (the diameter of whose cylindrical wick was 0.75 inch), by increasing the flame. The first experiment was made for the sake of comparison, in order to estimate the effect of the glass chimney; the instrument was one having the bulbs at the same height; the sentient bulb coated with Indian ink.

Number of experiments.	Particulars.	Rise in 1 min. corrected for adventitious light.			
		Both bulbs exposed = $t, l + h.$	Plain bulb screened = $l$	Hence $h.$	$\frac{l}{h}$
3	{ Argand lamp 7 inches from bulb with chimney }	15	28	13	$\frac{1}{0.8}$
3	{ Ditto no chimney..... }	18	30	12	$\frac{1}{0.6}$
2	{ Ditto no chimney..... }	14	31	17	$\frac{1}{1.2}$
2	Wick increased.....	29	40	11	$\frac{1}{0.38}$
1	More increased.....	30	52	22	$\frac{1}{0.7}$

It is obvious that there is a limit beyond which increasing the wick does not produce more complete combustion.

Of several other experiments tried on flame, one case regards the alteration which takes place in a flame as exhibited in the simple experiment of placing salt in the wick of a spirit-lamp; the effect being increased also by diluting the spirits with water. (See Dr. Brewster's paper on a Monochromatic Lamp, Edinb. Phil. Journ. No. 19, p. 123.) This experiment gave the following results. The instrument employed in this and all the subsequent experiments was a small photometer, having its bulbs in the same vertical line.

Number of experiments.	Particulars.	Rise in 30 seconds.		
		$l$	$l + h$	$\frac{l}{h}$
3	{ Flame of spirit lamp. Distance 1.5 inch..... }	2	12	$\frac{1}{5}$
3	{ Spirits diluted, and salt placed in the wick: the flame smaller. }	2	7	$\frac{1}{2.5}$

(8.) Count Rumford found that, when by employing many flames near each other, the temperature of the flame was increased, the light given out increased in a much greater proportion. (See Phil. Trans. 1820, Part I. p. 22; Davy's Elements of Chem. Phil. p. 224.) If the simple heat radiated increases in a proportion not greater than the temperature of the flame, we shall here observe the same increase of ratio between the radiant heat and the light as in the preceding instances.

This point I proceeded to examine in the following set of experiments, in which I compared by the same method as before, the effects of light and heat produced from a single flame, and from the juxta-position of flames.

Number of experi- ments.	Particulars.	Rise in 30 seconds.			$\frac{l}{h}$
		$l$	$l + h$	$\therefore h$	
5	{ Flame of wax candle, 3 inches distance ..... }	2	5	3	$\frac{1}{1.5}$
3	Two flames coalescing ....	5	9	4	$\frac{1}{0.8}$
4	Another set. ....	7	10	3	$\frac{1}{0.4}$
3	Distance $1\frac{1}{4}$ inch .....	5	19	14	$\frac{1}{2.8}$
3	Two flames coalescing ....	12	28	16	$\frac{1}{1.3}$
2	{ Distance 3 inches. Disc in 1 minute ..... }	10	25	15	$\frac{1}{1.5}$
	Two flames coalescing ....	27	44	17	$\frac{1}{0.6}$

In all these cases the increase of the ratio between the effects of light and those of simple heat is very conspicuous; and it appears both from the results of Count Rumford, &c. as well as these, that the increase of light is in a ratio greater than that of the increase of temperature; the effects of light being in these experiments more than doubled when two flames were united, whilst the heat radiated was less than doubled.

(9.) I now extended the inquiry to the radiation from metal at different stages of incandescence. For this purpose I employed a mass of iron of a cylindrical form, about six inches long and 1.5 diameter; heated to the brightest point which a common fire could communicate, and suspended vertically. The photometer was exposed to it at seven inches distance, placed opposite to the middle point of its length. In the first sets of these experiments, I observed the effect of light, using the glass case, and therefore could make no comparison of the effect of the light with that of the heat. I am well aware that these numerical results can only be regarded as rough approximations; yet they will give some idea of the different law followed in the progression of the two parts of the effect. The value of  $(l + h)$  may have been somewhat too small from a trifling heating effect of the small screen on the lower bulb.

Incandescent iron. Rise in 30 seconds. Distance 7 inches.						
Minutes from commencement.	Exp. 1.		2		3	4
	$l$ (case)	$l + h$	$l$ (case)	$l + h$	$l$ (no case)	$l$ (no case)
0	12		8		9	10
1		36		36		
4	5		4			
5		20		22		
8			1			
9				10		

(10.) In the following set of experiments, the effect of light was observed with the instrument in its glass case, with an additional screen of plate glass several times replaced; for ( $l + h$ ) there were two screens to the lower bulb; the outer one several times replaced.

Incandescent iron. Rise in 30 seconds. Distance 7 inches.						
Minutes. Seconds from commencement.	$l$ (glass case and screen).			$l + h$ (no case)		
	Exp. 1	2	3	1	2	
0	18	12	16	36		
2:30		4		22	22	
5	2	3	2	18	11	
7:30	1	0		18	10	
10	0	0	0	14		
15				8		
17:30				5		
20				4		

(11.) I subsequently made a similar set of observations with a ball of iron two inches in diameter. In the former case, the values of ( $l$ ) and of ( $l + h$ ) were not so taken as to be comparable. In the present instance I attempted, by obtaining both results under similar circumstances, to deduce the value of ( $h$ ) and the ratio as in former experiments. For this purpose, to obtain ( $l$ ) the instrument was employed without its case, but with three screens of plate glass; and, as before, for ( $l + h$ ); the nearest screen was 1.5 inch from the bulb; here there might be some small cooling effect. After the experiment, the outer screen alone was found at all heated.

Incandescent iron. Distance 6 inches.

Exp. 1. Rise in 30 seconds.

Minutes from com- mencement.	$t + h$	$\therefore h$	$\frac{t}{h}$
	34	28	$\frac{1}{4.7}$
	25	23	$\frac{1}{11.5}$
	10	10	
	8	8	

Exp. 2.

0	5	37	32	$\frac{1}{6.4}$
2	1	29	28	$\frac{1}{28}$
4	0	12	12	
6	0	10	10	

(12.) For the sake of comparison, I here again repeated the observation of the light with the case: the indications were:

Case. Case and two screens.

(1) (2) (1) (2)

Rise in the first 30 seconds . . 8° 11 . . . . 8° 9

(13.) The general inference from these experiments is, that observing the progress of the radiation from a hot mass of metal, beginning with the heat of luminosity, we find the radiant heat increasing, and the heating power of the light distinct from it increasing also, but the former in a less ratio than the latter.

Thus it would appear that this same law is followed in all the different cases of luminous hot bodies here considered, in proportion to the density of the flame, to the completeness of combustion, to the coalescing of several flames, and to the degree of ignition in metal.

The heating power of light increases in a higher ratio than the simple radiant heat which accompanies it.

(14.) The fact which I conceive is established in the first part of my experiments, viz. that in the radiation from luminous bodies, simple radiant heat exists distinct from the light and its heat, appears to me of some importance in regard to the validity of that theory which asserts that heat is merely light in a state of combination. According to that theory, as the temperature of a body is raised, it begins to give out the "igneous fluid" in a free radiant state: this at first is simple radiant heat, but by degrees its properties and intensity become altered, and it begins to act upon our organs with an illuminating effect; but is liable

to absorption again from bodies on which it impinges, in proportion to the darkness of their colour, and thus becomes heat, displaying its effects as temperature.

The views to which I have been led as to the distinct nature of the two parts of the total heating effect, so far tend to disprove this theory, that we here evidently perceive a very considerable portion of the radiant matter not at all converted into light, but merely increased in intensity.

If, therefore, we still adhere to the supposition that light is only heat in a different state, we must so far modify the hypothesis as to admit that only a part of the igneous fluid undergoes this change. But here we must further ask, whether such an admission can be made in consistency with the other parts of this theory, or even with its fundamental principles. For this purpose we must take a brief review of its leading features, and the grounds on which it is built.

(15.) Prof. Leslie, in his Inquiry into the Nature of Heat, p. 150, maintains the opinion of the materiality of light, and of its existence in actual combination with bodies. He then examines the phenomenon of its absorption as connected with reflection, &c.; he attributes to light in its state of combination the heating property, from which he is led to the conclusion, that "heat is light in a state of combination," p. 162.

The ground upon which he adopts this theory is this; having come to the experimental conclusion that "heat is an elastic fluid extremely subtle and active," he asks (p. 150), "Is it a new and peculiar kind of fluid, or is it one with which, from its effects, we are already in some manner acquainted? If any such can be discovered that will strictly quadrate with the phenomena, the spirit of true philosophy which strives to *reduce the number of ultimate principles*, would certainly persuade us to embrace it. But in searching further, we may, perhaps, educe direct proofs of identity;" and then, from a comparison with the effects of light before mentioned, he concludes their identity. We may, however, be permitted to ask, whether, to suppose two existences (in some particulars at least), possessing such very different properties as light and heat do, to be merely the same substance in different states, is not rather departing from the general simplicity of natural causes; and supposing a new sort of relation between two existences with which in other parts of nature we are unacquainted, should we not be more in the true spirit of inductive philosophy, if, admitting the distinct existence of light and heat we sought to explain, the facts of the one being apparently produced by the other, according to some laws already known to act in the constitution of things.

Prof. Leslie shows (p. 175), that all rays of light from whatever source must issue from that source with the same identical celerity. "It hence appears," he observes, "that light must

derive its projectile impulse from the sole operation of its peculiar elasticity while in the state of heat." "Its motion," he then shows, "is exactly similar to that with which an expansive fluid will rush into a vacuum." He concludes this profound investigation by remarking, that "We are forced to suppose that when bodies discharge light, they are thrown into a sort of convulsive state, having their adhesive attraction to it affected by momentary intervals of suspension, during which fits, the luminous particles, being set free, are projected by their own intrinsic repulsions. Without admitting this hypothesis, it seems impossible to explain the equality of motion which belongs to every species of light, however variously combined with different bodies as constituting heat, it is emitted from them all with the same rapidity; and such we have seen is the remarkable property of an expansive fluid when liberated." (P. 177.) He then proceeds by a highly curious computation to calculate the elasticity of light, the weight of combined light on heat, the waste of luminous matter in the sun, and other points connected with these.

In thus explaining the projection of light from bodies, its existence as an expansive and elastic fluid is easily admissible, and appears conformable with all we know of its properties. But the hypothesis just quoted respecting the state into which bodies must be brought in order to discharge it does not seem susceptible of experimental proof.

Admitting the reasoning from which the phenomena of its emission are explained on the supposition of its being an elastic fluid, we may ask is it necessary to suppose in order to its being thus emitted that it is identical with heat? Should we not avoid the part of the hypothesis last alluded to, if we supposed the light to exist in combination with the solid substance in the same way as gaseous fluids are known to exist in combination with such substances, and that by the operation of the heat which is employed in raising the body to the temperature of luminosity, the light before in combination is made to assume its elastic fluid state, and then is projected from the body according to the mathematical principles which Prof. L. has just before laid down.

From this review, we shall be prepared to perceive how the circumstance before adverted to presents a serious difficulty to the hypothesis. If the heat of a body be converted into light, owing to the action of the causes here explained, we may ask how then it can happen that only *part* of the whole quantity of heat combined with the body is thus changed into light? or why the increase of elasticity only takes place in part of the combined fluid, and not in the whole; for the unaltered portion still continues to be radiated as heat, but is neither converted into light,

nor in any way altered in its properties so as to approach the nature of light. It seems to be impossible to conceive that the mere continued and increased action of one cause, or a fluid of one simple nature can change a portion of it into a new substance, and yet leave a very considerable part in its original state. Nor can the difficulty be diminished by supposing the heat to be a compound of two different species, one convertible into light, and the other not; because, as we have seen in these experiments, in the same body the proportions of the two will be constantly varying with the increasing intensity of ignition.

(16.) After all it becomes a question, does any part of the simple heat disappear so that we can suppose it either converted into light, or in any other way changed in its properties? It does not appear to me that this has been in any way established either by the supporters of the theory just alluded to, or any other experimenters; yet its investigation is clearly a point of importance. If it should be shown that it does not take place, this theory (independently of the objections just urged) would entirely fall to the ground. If it should appear that some such phenomenon does take place, the above objections would not be in the slightest degree removed; and we might then, perhaps, have some ground for a more correct and inductive view of the subject.

(17.) This was one principal point I had in view in these experiments, and I conceive to be by them sufficiently shown, that a portion of the heat which we know upon independent grounds is generated, is actually lost, or does not appear either as heat of temperature, or in a radiant form.

The general result of my second set of experiments is, that at first the heating of a body causes it to continue radiating heat in a proportion which is nearly that of the increase of temperature. At a certain point which we call the temperature of luminosity, light begins to be given out, possessing a heating power when absorbed again; the light estimated both by this power, and generally also by its illuminating effect, continues to increase; whilst the simple radiant heat, distinct from it, continues to increase also, but in a less ratio than the light.

The radiant heat probably *tends* to increase in a certain ratio to the elevation of temperature; at the same time (from the peculiar constitution of bodies) an increasing quantity of it is continually abstracted, or ceases to appear as radiant heat, and this loss corresponds to the increase of heating power in the light.

This law applies to the case both of the same body at different stages of ignition, and to the comparison of different luminous bodies, as different flames, which have been shown to have different temperatures of luminosity, and which on arriving at a

certain stage of combustion continue at the degree of ignition which belongs to that point without further increase, that degree being different for different combustibles.

It appears probable; if we extend the analogy from what we already know, that the general law is, that in proportion to the completeness of the combustion, more light and proportionally less heat are radiated; and it seems natural to suppose that a greater energy of action would rather cause the heat to be employed in evolving light, than simply to radiate away.

In the sun's rays it has been shown that the light produces the whole heating effect; hence if the origin of the solar rays be from any process similar to combustion, it must be analogous to the most perfect kind of combustion.

Mr. Brande has shown (*Phil. Trans.* 1820, Part I.) that the galvanic light approaches more in its chemical properties to the nature of the solar light than that from any other source.

(18.) If any doubt should remain as to the actual disappearance of a portion of heat, let us only advert to the instances afforded in the above experiments. In increased intensity of combustion, a proportional increase of heat must be generated; but from the more intense combustion a greater increase of heating power is communicated to the light than is exhibited in the radiant heat. This increase, therefore, consists of heat derived from the hot body no longer forming heat of temperature, and no longer radiating as heat; but combined in a peculiar way with light.

Again, solid particles volatilized in a flame acquire temperature from it; but they hence give out much more heating light, but not as much more radiant heat.

On the uniting of different flames, the same thing is most palpably shown. The two flames united give out less than the sum of their separate heats, and more than the sum of their separate heating powers of light; the latter must be increased at the expense of the former. The heat disappears either as temperature or as radiant heat.

From the experiments on incandescent metal, we might deduce exactly the same conclusion.

(19.) That the extrication of light is in most cases owing in some way to the agency of heat has been long an established opinion. Thus Mr. Morgan (*Phil. Trans.* 1785, No. 11) considers light as a substance united to other bodies by peculiar attraction, and separated by having that attraction overcome by heat. Blue rays he conceives to have the least, and red the greatest affinity, and consequently the former are first separated by the heat, and it is not till the last stage of combustion that red light is given off. He considers the increased generation of heat to produce a corresponding increase in the evolution of light.

The experiments of Mr. T. Wedgwood also tend to the same point. (Phil. Trans. 1792, No. 3.) He considers the light produced by attrition to be evolved by means of the heat generated.

The agency of heat in causing the evolution of light is clearly recognised by Count Rumford, in his experiments on the coalescing of flames. He conceived the increase of light to be owing to the proximity of the flames "so as to communicate heat." (See Mr. Brande's paper on Combustion, Phil. Trans. 1820, Part I. and Sir H. Davy's Chem. Phil. p. 224.)

But there are cases in which light is extricated, where it does not appear that any elevation of sensible temperature is necessary to its production. Such are the instances of phosphorescent animals, of the light generated during putréfaction, &c. Any theory of the subject ought, therefore, to be sufficient to explain, not only how the heat acts in evolving the light in the former cases, but how the same cause can produce the effects as in this latter case when the temperature is not increased. If then any theory should at once embrace these two apparently very opposite cases, it would probably be considered a strong argument in favour of it.

(20.) We have not any precise ideas as to the mode in which the heating effect which takes place whenever light is absorbed, is produced. The theory which asserts that the light is *transformed into* heat is a wholly gratuitous assumption; it lays a great claim to simplicity of principle; but this is, perhaps, more apparent than real. The simplicity of any hypothesis, considered as an explanation of phenomena, depends not solely on the absence of complicated combinations, but also on its analogy to some well established principles on which other similar classes of phenomena are explained. Thus it is easy to say, and to conceive, that light on absorption is converted into heat, or exists under a different form; but this, besides being a mere assumption answers very little purpose; and scarcely brings us one step nearer to an explanation of the phenomena than we were before; in other words, it does not exhibit them in any such point of view as makes them analogous with any other class of phenomena. Not only, however, is this hypothesis *wanting* in the characteristic just mentioned, but it also appears to me to be fairly chargeable with being positively at *variance* with all established analogy.

According to this theory light is a peculiar and extremely subtle species of matter which, in its ordinary state, is distinguished only by the property of illumination, but on being absorbed by bodies, enters into combination with them, and is changed into a new substance, or continues to exist under the form of another sort of matter, which is heat.

Thus we have an extremely subtle sort of matter undergoing an immense degree of condensation, and becoming a component

part in a solid body; but, according to all analogy, we should here expect that in doing so, it would give out a considerable quantity of latent heat, which would be rendered sensible in raising the temperature of the body. The above theory, however, will not allow of such a view of the matter; for the heat produced is here the *substance itself* which is united to the body, and not the *result* of that union. Moreover that a substance should at once be in combination with another body, and at the same time act upon other bodies as if it were free and uncombined, is still further contrary to analogy.

If then we assume the materiality of light, and wish to take such a view of the mode in which its heating effects are produced as shall at once make the smallest assumption, and be most analogous with other phenomena, we must seek for some other hypothesis than that just alluded to.

(21.) It may be considered as established that the portion of light which is not reflected from a surface undergoes an absorption, and changes its state; whether it form a true chemical combination with the body is a point which is probably beyond our means of investigation. It is, however, certain, that immediately on the absorption taking place, heat is produced in the body; but since we are in ignorance of the nature of the combination formed by light with the body, it is surely a most unwarrantable assumption to say, that the combined substance *is heat*.

On the other hand, seeing an extremely subtle substance enter into combination with a solid body, and finding heat produced in that body, what idea can we more naturally and indeed unavoidably form, than that the increase of temperature is here, as in all other cases, occasioned by the giving out of latent from the absorbed substance.

(22.) In conformity with the phenomena of the changes of state in all other sorts of matter, we here readily perceive that, first, when light is absorbed and enters into combination with common matter, heat is given out, and different degrees of heat by different species of light; secondly, light is not generated without a certain degree of heat. All bodies at some temperature become luminous, and after arriving at a certain temperature, an excess of heat, which continues to be generated, is employed in giving the form of light to some particles of the body by becoming latent in the elastic matter into which they form.

(23.) The view which I have taken of the subject appears to me to be one to which we are directly led by the phenomena of the experiments. It has long appeared to me a very vague opinion, as well as one very much at variance with all analogy, to say, that "light and heat mutually evolve each other," or that they are modifications of the same substance. The view I have adopted exhibits the circumstances of the union and separation of these agents in a way perfectly conformable to other

physical changes. We universally observe a tendency in nature to avoid a multiplicity of causes, and to produce a variety of different effects by the intervention of one and the same cause differently modified. In strict conformity with this principle, the explanation I have attempted indicates a beautiful extension of the great law of *latent heat*, long since so successfully applied to the investigation of the different states in which matter exists, and to the phenomena of the combination and separation of different forms of matter; and if any agent or principle in nature should exhibit phenomena exactly analogous to those presented by the changes of ordinary matter in relation to heat, we may without impropriety describe such phenomena by analogous terms, and speak of the absorbing or giving out of latent heat by such agents, without assuming any particular hypothesis respecting their materiality. The observance of such analogies holding good with these agents or principles, would, however, be so far a presumption in favour of their materiality.

We have become acquainted with matter in three different forms, or states, solids, liquids, and gases; but there is nothing in nature to prevent the supposition that there may be other states in which matter is capable of existing, which may form an extension of this series at either end, and owe their difference to the same cause, viz. the possessing or losing a certain quantity of latent heat.

May not then light be one of such forms of matter? a term in the series occupying a place beyond gaseous bodies (though not necessarily next to them), and owing its peculiar form to the absorption of a certain quantity of latent heat.

It would be easy to go on without limit in noticing the analogies which might be found between the properties of elastic fluids and those which might belong to an order of bodies beyond them in the scale of latent heat; but upon these speculations I forbear entering. That the analogy holds good in respect to latent heat is all that I am now concerned to maintain; and this, I think, has been fully made to appear from experimental deduction.

## ARTICLE VI.

*Analysis of Tartarized Antimony.* By R. Phillips, FRSL. & E

ACCORDING to Dr. Paris (*Pharmacologia*, vol. ii. p. 64), this "saline body was first made known by Adrian de Mynsicht in his *Thesaurus Medico-Chymicus*, published in 1631; although it appears probable that the preparation was suggested by a treatise, entitled '*Methodus in Pulverem*,' published in Italy in 1620." Long as this medicine has been employed, no regular analysis of it, as far as I can learn, was attempted until 1801,

when M. Thenard published the result of his analysis in the 38th volume of the *Annales de Chimie*, p. 301.

Before, however, I offer any observations on this analysis, I shall state the atomic weights of the constituents of tartarized antimony. Bitartrate of potash consists of 2 atoms of acid  $66 \times 2 = 132$ , and 1 atom of potash 48, and consequently the atomic weight of the anhydrous bitartrate is 180; these are the results of Dr. Thomson's experiments, and I believe them to be perfectly accurate; according to Berzelius's table of equivalents, the proportions of acid and base are nearly as above given, but he states the salt to contain 4.74 per cent. of water, and, therefore, 180 of the anhydrous salt must unite with 8.95 of water; so nearly 9, that, we may conclude, if the experiments be correct, that the crystals of bitartrate of potash contain 1 atom of water. Dr. Thomson, however, in a very important work which he has very recently published,\* considers this salt to contain 2 atoms of water; but, for reasons which I shall now state, I still consider the determination of Berzelius to be correct: having prepared some pure bitartrate of potash, I suffered it to dry by exposure to the air; 189 grains of this salt, containing, of course, hygrometric moisture, were boiled in water with 54 grains = 1 atom of dry carbonate of soda; the solution was slightly alkaline, but upon adding 3 grains of bitartrate of potash, it reddened vegetable blues strongly; now if the salt had contained 2 atoms of water, it would have required more than 198 grains of bitartrate of potash, instead of less than 192, to have supersaturated 54 of carbonate of soda. I heated some crystals of the common bitartrate at a temperature but little below that required for their decomposition; they lost only 0.95 per cent. and I, therefore, conclude that this salt cannot be rendered anhydrous by heat. With respect to the atomic weight of antimony and its compounds, I also adopt Dr. Thomson's numbers, viz. 44 for the metal, 52 for the protoxide, and 60 for the peroxide and sulphuret.

M. Thenard analyzed tartarized antimony in the following manner:—100 parts of the crystals were subjected to heat, by which they lost 8 parts of water, the remaining 92 parts were dissolved in water, and the oxide of antimony was precipitated by sulphuretted hydrogen; 50 grains of dry precipitate were obtained, which were calculated to contain 38 parts of oxide, such as it exists in the salt: by means of acetate of lead, 100 grains of tartrate of lead were procured, which are estimated to contain 34 of tartaric acid; and, lastly, 100 parts of the salt being treated with nitric acid, there were obtained 30 of nitrate of

\* An Attempt to establish the first Principles of Chemistry by Experiment. By Thomas Thomson, MD. FRS. Regius Professor of Chemistry in the University of Glasgow, &c.

potash, and these were calculated to contain 16 of potash. From these results, M. Thenard concludes, that tartarized antimony is composed of

Tartaric acid . . . . .	34
Potash. . . . .	16
Oxide of antimony. . . . .	38
Water . . . . .	8
Loss. . . . .	4
	<hr/>
	100

M. Thenard asserts, that bitartrate of potash contains more tartrate of potash than is necessary, to saturate the tartrate of antimony, and he states that this excess of the salt remains in the mother water. This is certainly a mistake, for crystals of tartarized antimony are procured from almost the last drop of the solution.

Now if, as already supposed, 2 atoms, or 132 of tartaric acid combine with 1 atom or 48 of potash to form the bitartrate, it is evident that M. Thenard's analysis must be incorrect, for in that we find the quantity to be 34 to 16, or 132 to 62·11; nor will the error be rectified by supposing the 4 parts of loss to be tartaric acid, for the proportion even then would be 132 to 55·57.

In his *Essai sur la Théorie des Proportions Chimiques*, Berzelius has given the following formula to represent the constitution of what he terms tartras kalico-stibicus,  $3 \ddot{\text{K}} \ddot{\text{T}}^2 \text{Aq}^2 + 4 \ddot{\text{Sb}} \ddot{\text{T}}^3 \text{Aq}^1$ . These symbols I have not attempted to decypher, nor was it necessary to do so, for the composition of the salt is stated in 100 parts as follow :

Tartaric acid . . . . .	53·20
Potash . . . . .	12·53
Oxide of antimony . . . . .	27·10
Water. . . . .	7·17
	<hr/>
	100·00

Examining these results on the same principle as the analysis by M. Thenard, it will appear to be also incorrect, for if 132 of tartaric acid combine with 48 of potash, 53·20 should unite with 19·34 instead of 12·53 as above quoted; the quantity of oxide of antimony is also very incorrectly given, and the only statement which approaches exactness is that of the quantity of water.

Dr. Gobel, in Schweigger's *Journal (Annals, vol. viii. p. 151, N. S.)*, states the results of his analysis to be,

Tartaric acid . . . . .	45.00
Potash . . . . .	9.80
Oxide of antimony . . . . .	42.60
Water. . . . .	3.75
	<hr/> 101.15

Dr. Gobel considers the atomic constitution of the salt to be equivalent to 1 atom of tartrate of potash, 2 atoms of subtartrate of antimony, and 2 atoms of water; but as 132 of tartaric acid require 48 of potash, 45 must unite with 16.36 instead of only 9.8 as stated: this analysis is also incorrect as to the quantity of water; but that of the oxide of antimony is not very far from the truth.

Mr. Brande, in his *Manual of Chemistry*, vol. iii. p. 85, states that "Mr. Phillips has shown that emetic tartar consists of 100 supertartrate of potassa + 66 protoxide of antimony. If we consider it, with Dr. Thomson (*System*, ii. 670), as a compound of 2 proportionals of tartaric acid, 2 of protoxide of antimony, and 1 of potassa; or as containing 1 proportional of tartrate of potassa and of subtartrate of antimony, its components will stand thus:

Tartaric acid $62.5 \times 2 =$ . . . . .	125
Protoxide of antimony $52.5 \times 2 =$ . . .	105
Potassa = . . . . .	45
	<hr/> 275"

Now it is to be observed that my statement respecting the solvent power of supertartrate of potash (*Experimental Examination*, p. 85), is that when equal parts of the salt and oxide are boiled together in water, 70-100ths of the oxide are dissolved. It is, however, to be remarked, that the composition of tartarized antimony cannot be inferred from the oxide dissolved by the common bitartrate of potash, for it always contains about 6 per cent. of tartrate of lime. In addition to this, it must also be proved, either that the bitartrate of potash and the antimonial salt contain no water at all, or only that quantity which previously existed in the bitartrate.

In his *Manual of Pharmacy* lately published (p. 254), Mr. Brande observes, the "composition has been variously stated, and experiments are still wanting to demonstrate the relative proportions of its component parts: its most probable composition is 1 proportional of tartrate of potassa, and 1 of subtartrate of antimony, or,

Tartrate of potassa . . . . .	= 115
Tartaric acid . . . . .	= 67
Protoxide of antimony ( $53 \times 2$ ) . . . . .	= 106
	<hr/>
	288"

This, however, is an incorrect view of the subject, for it supposes the salt to be anhydrous, which it is not, and the quantity of oxide of antimony assigned is too small. Indeed having mentioned the results of my experiments to Mr. Brande, he has stated the composition differently in the table of equivalents, adding in a note, "According to Mr. R. Phillips: in the text at p. 254, the quantity of protoxide is underrated by one proportional." Still, however, the statement is incorrect, as I shall presently show.

The quotation just made from Mr. Brande's Manual of Chemistry contains the opinion of the atomic constitution of tartarized antimony expressed by Dr. Thomson in his System. In his new work, already alluded to, Dr. T. observes (vol. ii. p. 440), "No accurate analysis of this useful salt having been hitherto made, I took the following method of ascertaining its constituents: 50 grains of picked crystals of tartar emetic were dissolved in distilled water, and a current of sulphuretted hydrogen gas passed through the liquid as long as any precipitate fell. The hydrosulphuret of antimony thus obtained, when dried in the open air, weighed 42.21 grains; but when heated in a glass tube, water was driven off, and a black matter remained, which weighed 24.59 grains, and which was sulphuret of antimony, equivalent to 18.032 grains of antimony, or 21.31 grains of protoxide of antimony.

"The liquid thus freed from antimony was evaporated cautiously, and a quantity of bitartrate of potash obtained, which weighed 28.69 grains. But the integrant particle of bitartrate of potash weighs 24.75; and  $28.60 : 24.75 :: 21.31 : 18.384$  = the protoxide of antimony united to an integrant particle of bitartrate of potash. Now the protoxide of antimony weighs 6.5, and  $6.5 \times 3 = 19.5$ ; this is a little more than I actually found, because part of the sulphuret in my experiment adhered to the glass tube, and could not be collected without loss. From this experiment, which I thrice repeated, I have no doubt but the constituents of tartar emetic are,

2 atoms tartaric acid . . . . .	16.5
3 atoms protoxide of antimony . . . . .	19.5
1 atom potash . . . . .	6.0
2 atoms water . . . . .	2.25
	<hr/>
	44.25"

I shall now proceed to state the results of my analysis, having already quoted from Mr. Brande, that I had obtained,

as Dr. Thomson has, 3 atoms of protoxide of antimony from this salt.

A. 100 grains of brilliant small crystals of tartarized antimony reduced to powder were heated during eight hours at the temperature of  $212^{\circ}$ ; they lost only 2.1 grains; but as bitartrate of potash retains the water of crystallization when exposed to a much greater heat, I subjected 100 grains of tartarized antimony reduced to powder to a higher temperature. Taking the mean of several experiments, I found that the salt lost 7.38 per cent. by several hours' exposure to a sand heat. When one portion, which had lost 7.1 per cent. in this way, was heated by a spirit-lamp, so as to suffer a further diminution of 0.1, it was decomposed, and becoming of a brown colour, it emitted the smell of decomposing tartaric acid. I consider, therefore, 7.4 per cent. as the quantity of water.

B. I attempted to ascertain the quantity of oxide of antimony in two different modes. First, I decomposed a solution 100 grains of the crystals by carbonate of soda, assisted with heat; the mean of two experiments gave 41.35 per cent.; but the alkalis being imperfect precipitants of antimony, I treated the solution afterwards with sulphuretted hydrogen, which gave a mean of 2.8 of precipitate dried at a moderate temperature, and which I conceived to be hydrosulphuret of antimony composed of 1 atom of sulphuretted hydrogen  $17 + 1$  atom protoxide of antimony  $52 = 69$ ; if then 69 contain 52 of oxide  $2.8 = 2.11$ , which, added to 41.35, gives 43.46 as the quantity contained in 100 parts of the salt.

C. After this, and in order to confirm the above statement, I treated two solutions each of 100 grains of tartarized antimony with sulphuretted hydrogen gas; the precipitates after being dried on a sand heat gave a mean of 51.25 grains. Now, if we admit, as above supposed, that this precipitate is hydrosulphuret of antimony, and of which it possesses the appearance, tartarized antimony contains only 38.6 instead of 43.46 of protoxide of antimony as by Experiment B, for  $69 : 52 :: 51.25 : 38.6$ . It may be further observed that the quantity of precipitate obtained in C, as well as the inference as to the quantity of oxide which it contains, agree very nearly with the previous determinations of Thenard.

These discordant results, repeatedly obtained, puzzled me exceedingly, and I adopted two modes of determining the quantity of oxide existing in the dried hydrosulphuret.

D. I dissolved 45.6 grains of protoxide of antimony in a solution of bitartrate of potash, and then precipitated it by sulphuretted hydrogen, after washing and drying in the same mode as before, the precipitate weighed 52.8 grains; and, as it had the appearance of being an hydrosulphuret of antimony, I suspected that it was subhydrosulphuret consisting of 1 atom of sulphuretted hydrogen, and 2 atoms of oxide of antimony, on

which supposition I ought to have obtained 53.05 of precipitate instead of 52.8; and it will be observed that supposing this to be the true constitution of this substance, the results of B will agree with those of C as nearly as 43.46 to 44.04, for subhydrosulphuret of antimony must consist of  $52 \times 2 + 17 = 121$ , and  $121 : 104 :: 51.25 : 44.04$ .

E. Further to examine this view of the subject, I dissolved 100 grains of sulphuret of antimony and 100 grains of the precipitate in question in separate and equal quantities of muriatic acid, and decomposed the solutions with similar portions of water: the precipitate from the sulphuret weighed 86.7 grains, and that from the hydrosulphuret 87 grains; and as in 1 atom of subhydrosulphuret, 17 of sulphuretted hydrogen, and 16 of oxygen = 33, would supply the place of 2 atoms of sulphur = 32 in 2 atoms of sulphuret of antimony, it is evident that equal weights of these compounds, should yield nearly equal quantities of precipitate by solution in muriatic acid and the effusion of water.

F. The nature of this precipitate was then examined by heating 50 grains in a small flask by a spirit-lamp, and I found to my surprise, that it was readily converted into black sulphuret of antimony, losing only 1.2 grain of water. It appears, therefore, that instead of a subhydrosulphuret as I had suspected, the precipitate was sulphuret, containing a small quantity of hydrosulphuret, but yet sufficient to give so much colour as to conceal the nature of the sulphuret. The difficulty of the case was increased by the fact already alluded to, viz. that 2 atoms of oxygen and 1 atom of sulphuretted hydrogen are so nearly equal in weight.

G. As then 50 of the precipitate contain 48.8 of sulphuret of antimony, 51.25 the whole quantity obtained, (C) must contain  $50.02 = 43.35$  of protoxide of antimony, which is the quantity contained in 100 grains of crystallized emetic tartar.

We have thus obtained 7.4 as the quantity of water, and 43.35 as the weight of the protoxide of antimony; and having found, as already mentioned, that crystals of tartarized antimony are obtained even from the last portions of the solution in preparing the salt, the remainder of 49.25 will give the weight of the bitartrate of potash in 100 parts, or it consists of

Bitartrate of potash (anhydrous) . . . .	49.25
Protoxide of antimony . . . . .	43.35
Water . . . . .	7.40

100.00

Calculating its constitution, according to the weight of the atoms already mentioned, tartarized antimony will appear to be a compound of

		In 100 parts.
1 atom of bitartrate of potash .....	180	49.58
3 atoms of protoxide of antimony ( $52 \times 3$ ) ....	156	42.97
3 atoms of water ( $9 \times 3$ ). ....	27	7.45
	<hr/> 363	<hr/> 100.00

According to Dr. Thomson, as already quoted, the atomic weight of tartarized antimony is 354, differing from the above by 1 atom less of water.

## ARTICLE VII.

*On changing the Residence of Fishes.* By Nicholas Mill, Esq.  
(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

IN reading Dr. Mac Culloch's paper on "Changing the Residence of certain Fishes," in the 34th number of the Quarterly Journal, I was impressed with the importance of the subject to society, and I conceive that any facts which can be collected in corroboration of his statements, cannot fail to be interesting and useful. I therefore take the liberty to forward you some facts which have come under my observation respecting the habits of the salmon, and the likelihood of its being domesticated (if I may use the expression) in fresh water lakes and ponds. The salmon has many peculiarities in its history and mode of living which are common to most of the finny tribe that are inhabitants alike of fresh and salt water, and some which others have not. The salmon during certain months of the year is an inhabitant of fresh water rivers and lakes that communicate with the sea, and apparently for the sole purpose of depositing its ova or spawn. It then again betakes itself to the sea to recruit its strength and vigour, but, unlike other migratory fishes, its retreat cannot be traced; for it is a singular fact, that salmon have never been taken or seen in the salt water far from the mouth of some river, and there only at certain seasons of the year. The herring, the pilchard, the mullet, and the mackerel, may be followed from one sea to another, and from one creek to another, but not so with the salmon whose *hiding-place* is a mystery, yet to be solved by naturalists. The general opinion entertained upon this subject is, that they do not wander far from the shores of the fresh water rivers they frequent, from the well known fact that the *same* salmon will always return to the *same* river to deposit its spawn; and if they do migrate to the northern seas (which some naturalists maintain where they are found in abundance, but always, it must be remembered, in

communication with some fresh water lake or river), it is giving the species credit for greater sagacity than we usually find amongst the animal creation. In ascending fresh water rivers, they surmount the most surprising difficulties; weirs or dams of 15 feet in perpendicular height do not present an effectual obstacle to their progress, they are enabled by a spring or leap to pass them with ease. After having deposited their spawn, they become lean and covered with vermin, and fishermen assert, unless then suffered to return to the sea, they die; but this is one of the vulgar notions which it is my business to controvert. In order to ascertain whether sea water was necessary for the existence and growth of the salmon, I caught some of the fry of this fish as they were retreating to the sea with an artificial fly, and preserved them alive in order to transport them to a fish-pond; the dimension of which was about 30 yards square, with a clay bottom covered with mud; the depth of the water was from three to four feet, and the pond supplied with a running stream; when first caught they measured from the tip of the nose to the tip of the tail four inches: about twelve months afterwards, the pond was overflowed, when some of the fish, together with some trout, were left dry; they now measured in length eight inches, and assumed the shape and appearance of a lean salmon. If, therefore, the young salmon will live for twelve months in a space of thirty yards square, and three feet deep, and increase in size, the presumption is, that in water where it may range at large, and procure that food and situation which are most congenial to its habits, it would attain its usual size. In propagating this fish in fresh water, the greatest facility presents itself by transporting its ova or spawn, which is readily discovered in places frequented by salmon, to the places intended for its propagation, choosing as nearly as possible the same situation as that from which it has been removed.

The Chinese far exceed even the ancient Romans in the care they bestow upon the propagation of the finny tribe. At certain seasons of the year, they carefully collect all the ova as fast as it is deposited by the fishes to prevent its being devoured by the other tribes; they then procure some eggs, and after making a hole in each end, and blowing the inside of it through, the ova is introduced; and the ends being closed, the egg is placed in an oven of a certain temperature until the young fry nearly make their appearance, when the shell is broken, and the contents put into water warmed by the sun's rays. When the young brood procured by this means attain a certain size, a portion of it is applied for the purposes of food for the larger species of fish, and the remainder is destined for the table.

Your obedient servant,

N. MILL.

## ARTICLE VIII.

*On the Solar Spots.* By the Rev. J. B. Emmett.(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

*Great Ouseburn, March 20, 1825.*

I SEND a few remarks and observations on the solar spots, which may, perhaps, not be uninteresting to your readers.

In common with the generality of astronomers, I supposed the solar spots, which are carried round by the rotation of the sun with tolerable uniformity, to be visible and invisible for equal intervals of time : being a convert to the hypothesis of the late Sir W. Herschell, no doubt seemed to remain, since the equality of the periods follows of necessity. In the notes to the third volume of the Jesuit's edition of Sir Isaac Newton's *Principia*, the observations of Stannyan and Cassini are quoted, proving that the time during which the spots are visible, is twelve days, and that they are invisible 15; the learned commentators observe that of all the observations which they have seen, none prove that a spot has remained in view more than twelve days, or returned before the 15th. Kirchius observed one from April 26 to July 17, 1681; it was twelve days visible, and fifteen invisible. This observation is recorded in Chamber's *Encyclopedia*, the author of which work assumes this period as well known, and received generally by astronomers. In the *Phil. Trans.* 1704, the observations of Stannyan in 1703 are quoted; a spot which he observed for some days, was visible in the morning of May 23 (civil time), again at, and after noon, but then upon the edge of the disc; but at 3<sup>h</sup> it had entirely vanished; it was upon the edge June 7<sup>d</sup> 3<sup>h</sup>; again it was on the limb, and disappeared June 19<sup>d</sup> 2<sup>h</sup>. Now from May 23, 3<sup>h</sup> afternoon, to June 7<sup>d</sup>, 3<sup>h</sup> afternoon, is precisely 15 days; also from June 7<sup>d</sup>, 3<sup>h</sup> afternoon, to June 19<sup>d</sup>, 2<sup>h</sup>, is 11<sup>d</sup> 23<sup>h</sup>. In the *Phil. Trans.* 1767 (when telescopes had received great improvement), the Rev. S. (late Bishop) Horsley, from these phenomena, calculates the altitude of that part of the solar atmosphere which the spots occupy; the commentators of Newton instituted a similar problem; Mr. Horsley takes it for granted that the spots are visible for 12 days, and disappear during 15; whence it follows that the sun's radius being 1, the spots are at the distance 1,013767 from his center. Yet all more modern astronomers assume that the spots are concealed behind the disc, and appear upon it during the same interval of time. Besides the observations quoted, I might have advanced many others; yet I cannot find any which show the return of a spot before the fifteenth day, or continue it on the disc more than twelve.

Some may reply that the early observations may be erroneous

in consequence of the imperfection of the telescope, before the reflecting and achromatic ones were invented: however, we must bear in mind that Cassini, and some others quoted, were good observers; their telescopes showed the division of Saturn's ring—his be'ts—the spots on Jupiter, and shadow of his satellites—the spots of Venus and of Mars, as well as many other of the most delicate objects, some of which, as the spots of Venus, are not easily seen, even with our improved instruments; besides, the telescopes then in use could trace a spot to the very edge of the sun's disc; than which more cannot be desired, especially since they showed the entire structure of the spots, which requires distinctness of vision. Also I have lately constructed two aerial telescopes, precisely such as were formerly used; one has an aperture 1·7 inch, and focus 18 feet; the other an aperture 3 inches nearly, and focus near 50 feet: the former has powers of 35 and 70; the latter 95 and 190; in distinctness they are very nearly, if not quite equal, to an excellent Newtonian reflector of six inches aperture, which shows the quintuple belt and double ring of Saturn, with a power of 800, and showed the spots of Venus, Feb. 21, of the present year, with powers 120, 200, 400. The shape of the nuclei and structure of the umbrae of the solar spots are beautifully seen with all the instruments: indeed from observation I am certain, that the long focal lengths of the telescopes whose object-glass is a single convex lens, gives them an advantage of no small magnitude over every other instrument in certain cases. As I shall trouble you with some observations made with these instruments, when those I shall have had opportunity to make are sufficiently numerous, I shall dismiss the subject for the present, assuring those who will make trial of a good plano-convex lens of crown glass, of from 20 to 50 feet focal length, that they will find, under judicious management, a distinctness of vision possessed by few instruments.

These remarks I have been induced to make, to prove that I am convinced from actual and long continued observations, that this extraordinary phenomenon is not to be ascribed to imperfection of the instruments; nor can it be attributed to any want of care, for perhaps a more indefatigable and careful observer than Cassini never lived; and besides it is not a little remarkable, that until very recently the two periods were not considered to be equal, but were believed to be 12 and 15 days; yet no journal, nor observations that I have seen, have been advanced in support of the recently received opinion. Accordingly I have instituted a series of observations; but not having yet made a sufficient number to enable me to discover to what extent the period may appear to vary in consequence of the proper motion of the spots, I do not propose them as infallibly decisive; I wish to make them public rather for the sake of inducing other

astronomers to examine the phenomena. In the observations, I have received much assistance from my friend Dr. Wasse, who is a very good and careful observer.

A small spot disappeared between the 8th and 9th of December last, at the western limb; on the 9th, 1<sup>h</sup>, there was no trace of it, nor of the *feculæ*, which I had constantly seen about it. On the 24th, 1<sup>h</sup> 45', there was no trace of the same spot on the east limb; but on the 29th, the intermediate days being cloudy, it had advanced nearly half way over the disc. By this observation, the precise time is not ascertained; yet it follows from it, that the spot was invisible, at least 15 days.

*Feb. 4, 1825.*—I discovered a cluster of spots: on the 12th, 1<sup>h</sup> 30', the most easterly spot was very near the edge of the sun's disc; its breadth was extremely narrow; it was observed with a Newtonian reflector of six inches aperture, with powers of 70 and 120, until about 5<sup>h</sup>, when it was not more than its own breadth from the edge of the disc, which certainly was not 4" of a great circle. Now the apparent diurnal motion of a spot is 13°—20' at a mean rate; hence the *sine* of the arc described by a spot in 24 hours, being on the edge of the disc at the beginning or end of the time, is 25",8816; from which data I conclude, that the spot must have disappeared about Feb. 12<sup>d</sup>, 10<sup>h</sup> astronomical time. On the following morning there was no trace of it; therefore, the computed time of its disappearing cannot be far wrong. Feb. 28<sup>d</sup>, 1<sup>h</sup>, Dr. Wasse observed the same spot, with an achromatic by Dollond, of three inches aperture, and power about 150. The nucleus, or black spot, had just entered, and the eastern part of the umbra was coincident with the limb: its magnitude was certainly not 4"; hence it cannot have been on the disc above five or six hours; but I will suppose 12<sup>h</sup>; upon this hypothesis, it cannot have appeared before 27<sup>d</sup>, 13<sup>h</sup> 30', which leaves 15<sup>d</sup> 3<sup>h</sup> 30' for the time it remained invisible. The observations cannot be minutely accurate, because the spot came into view, and disappeared in the night; but calculating from the number of observations which I have made upon the solar spots, during several years, I am certain the error cannot exceed five or six hours. I have not employed a micrometer in calculating the short intervals of time during which the spots could not be observed, because since the extent of the radius of the path which the spots describe is not known, such measures cannot be used without involving hypothetical views; the only certain plan is to observe the spots when just upon the very edge of the disc, which may be done during the long days in the summer, and it is on this account that I wish to call the attention of astronomers to the subject, as early as possible. Had the time, which I calculate to be 15 days, been but half the sun's period, the difference would be

1<sup>d</sup> 15<sup>h</sup> 30<sup>m</sup>, a quantity far too great to be considered an error of observation.

The same spot disappeared 12<sup>d</sup> 2<sup>h</sup>; and having come into view Feb. 27<sup>d</sup> 13<sup>h</sup> 30<sup>m</sup>, leaves 12<sup>d</sup> 8<sup>h</sup> 30<sup>m</sup> for the time they were visible.

Whether this be the true period, or the spots have a proper motion of their own, subject to no known regular laws, which sometimes lengthens the time they are invisible (but then it ought as often to increase the time during which they are in view) must be determined by observing them when upon the very edge of the disc; and this will rarely be observed at the entrance and exit of the same spot. Should the period which I have deduced be proved to be the true one, Sir W. Herschell's hypothesis cannot be supported; for the luminous stratum forms what is called the sun's disc: below this are the opaque clouds, and below these the dark globe; therefore, the nucleus ought first to disappear, which is not the case. It certainly may be seen when the umbra becomes invisible by reason of its proximity to the edge of the solar disc: indeed in 1818 I traced a spot which was surrounded with a fine umbra to the very edge; when there was a fine line of light beyond the spot, both the nucleus and umbra were very distinct; about half the nucleus projected beyond the umbra towards the sun's center. According to his hypothesis, the nucleus should disappear first, and even before it comes very near the limb, which is contrary to observation; then the umbra should disappear upon the limb, and return after 13 $\frac{1}{2}$  days. His hypothesis does not explain the reason why the margin of the umbra is darker than the interior, nor the clouded mottled form which large umbræ commonly assume.

I do not presume to produce any hypothesis; their structure is very remarkable, and totally dissimilar to that of any objects with which we are acquainted. When viewed with a telescope of sufficient power and great distinctness, their form presents many inexplicable phenomena; they are best seen with a large aperture, and power of from 70 to 200. Two pieces of plate glass, each smoked on one side, glazed with the smoked sides towards each other, interposing a margin of card paper to prevent their touching, is a preferable screen to any of the coloured glasses; and if made sufficiently large, the glass is not liable to be split by the heat, an accident to which I have always found coloured glass fixed into a brass ring so liable, that I can rarely use such a glass more than two or three times, allowing a sufficient aperture to the telescope. The mottled appearance of the solar disc, first particularly noticed by Sir W. Herschell, may be constantly seen with a considerable aperture, if the telescope be very distinct; if a screen of very white paper be placed where the focal image is formed by a good lens of about 50 feet

focus, which focus is above four inches in diameter, as I judge by the eye, the whole surface is seen to be covered with ridges of different degrees of brilliancy; indeed I have never seen it so conspicuously as in this manner.

I remain, Gentlemen, yours very sincerely,

J. B. EMMETT.

P. S. In my last paper on the <sup>5</sup>Mathematical Principles of Chemistry, I have made calculations on the supposition that gravity in the ultimate particles of matter is proportional to the density and volume jointly. In making the selection of matter, I omitted a problem which ought to have been at the commencement, viz. that in this case, cohesion, and all corpuscular forces, may be produced; this follows as a corollary from the ordinary problem in every treatise on Fluxions, "To find the attraction of a corpuscle to a right line," or from Prop. 90, of Newton's Princip. vol. i.; by substituting an evanescent or elementary prism: then the force becomes infinite in contact, and indefinitely great, at indefinitely small distances. The corollary admits of two cases; first, where the force acts in the direction of the prism; secondly, where perpendicularly to it.—J. B. E.

## ARTICLE IX.

### *Proceedings of Philosophical Societies.*

#### ROYAL SOCIETY.

April 14.—M. Gay-Lussac was admitted a Foreign Member, and Henry Harvey, Esq. a Fellow of the Society.

The reading was commenced of a paper, entitled, "A Monograph on Egyptian Mummies, with Observations on the Art of Embalming among the ancient Egyptians; by A. B. Granville, MD. FRS.

April 21.—The reading of Dr. Granville's paper was continued.

#### LINNEAN SOCIETY.

March 1.—The readings of Dr. Hamilton's Commentary on the third part of the Hortus Malabaricus, and Messrs. Shephard's and Whitear's Catalogue of Norfolk and Suffolk Birds, were continued; and on

March 15, the reading of the latter communication was further continued.

#### ASTRONOMICAL SOCIETY.

April 8.—A paper was read, "On the Results of Computations on Astronomical Observations made at Paramatta, in New  
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South Wales, under the direction of Sir Thomas Brisbane, KCB.; and the application thereof to investigate the exactness of observations made in the northern hemisphere. By the Rev. John Brinkley, DD. FRS." Anxious to throw some new light on the subject of the discordance between the north polar distances of the principal fixed stars, as determined by Continental and English astronomers, Dr. Brinkley wrote to Sir Thomas Brisbane, to request his Excellency to make some observations at Paramatta. Sir Thomas immediately commenced the important labour; and on a series of three months' observations, from November, 1823, to February, 1824, communicated to this Society as well as to Dr. Brinkley, the Doctor has founded the computations and comparisons which are communicated in this paper.

The sum of the polar distances of a star observed in the two hemispheres ought to be exactly  $180^\circ$ , if both are correctly observed. Also, on the hypothesis that the mean refraction is the same in both hemispheres, we have an opportunity of ascertaining the united effects of refraction, instead of the difference between the refraction of a star near the pole and of a circum-polar star remote therefrom.

In regard to the distance between the north and south poles, by combining Dr. Brinkley's observations with those of Sir Thomas Brisbane, the result is that the mean of 141 south polar distances deduced from 141 of his observations, and applied to Dr. Brinkley's north polar distances =  $179^\circ 59' 58''.92$  or  $1''.08$  in defect. Dr. Brinkley's refractions were applied to the southern observations, using the *interior* thermometer. The same mean, obtained by using Mr. Bessel's north polar distances and computing by Mr. Bessel's refractions (*Astron. Fundam.*), using the *exterior* thermometer, is  $180^\circ 0' 1''.72$  or  $1''.72$  in excess.

1°. Among the observations are some by reflection. These afford us the means of determining the zenith point, and thence the distance between the zenith and polar points, or the co-latitude.

Co-latitude by <i>Canopus</i>	$56^\circ 11' 8''.63$
— <i>Sirius</i>	9,16
— <i>Pomalhaut</i>	9,95
Mean	$= 56 \quad 11 \quad 9,25$
Latitude	$= 33 \quad 48 \quad 50,75$

2°. The results of observations on both the solstices of 1822 appear to show the latitude of Paramatta =  $33^\circ 48' 42''$ .—(No. 37, *Der Ast. Nachrichten.*)

The observations of the Dec. solstice of 1821 give the mean zenith distance of the solstitial point, Jan. 1, 1822, =  $10^\circ 21' 2''.23$ .—(No. 20, *Der Ast. Nach.*)

The mean obliquity of the ecliptic, taking the  
 mean obliq. of Jan. 1, 1816} =  $23^{\circ} 27' 49''.21$  =  $23\ 27\ 47.06$   
 and secular diminution =  $43''$  . . . . .

Latitude  $33\ 48\ 49.29$

If we use Mr. Bessel's obliquity =  $23^{\circ} 07' 45''.66$ , the latitude will be =  $33^{\circ} 48' 47''.89$ .

The result of all the observations shows that Dr. Brinkley's constant of refraction ( $57''.72$ ) is as exact as can be desired, when the refractions are computed by the *internal* thermometer; also that, when computed by the *external* thermometer, Mr. Bessel's refractions require no correction worth notice.

A communication was also read from Colonel Beaufoy, inclosing a series of observations of Jupiter's satellites, at Bushey Heath, near Stanmore, between April, 1816, and December, 1824; and another series of observations of solar and lunar eclipses, and occultations of stars by the Moon, occurring in the same interval of time, from 1816 to 1824 inclusive. The eclipses of Jupiter's satellites are so recorded as to show the mean time at Bushey, mean time at Greenwich, and then the same as exhibited in the Nautical Almanac. The discrepancies between the results of observation and the Nautical Almanac are in some cases very considerable. Even with regard to the *first* satellite, the differences sometimes exceed a minute and a half in time; and with regard to the other satellites, the differences exceed 2, 3, 4, and in one case (July 15, 1818), *seven* minutes of time. In this case the discrepancy is the same with respect to the *Connaissance des Temps*. The others the reporter has not had leisure to compare.

The reading of Mr. Atkinson's paper on refraction was also resumed and continued.

#### GEOLOGICAL SOCIETY.

Jan. 21.—A paper was read, entitled, "On the Freshwater Formations recently discovered in the Environs of Sete (Cette), at a short Distance from the Mediterranean, and below the Level of that Sea;" by M. Marcel de Serres, Prof. of Min. and Geol. to the Faculty of Sciences of Montpellier.

The freshwater formations described in this communication have been examined by means of several wells sunk at about the distance of about three-fourths of a mile, and a mile and a half from the Mediterranean, near Sete, in the south of France.

A detailed account is given of the several strata passed through in the three different wells, and of the organic remains which they contained.

The strata are for the most part parallel, and nearly horizontal. From the sections it appears, there are two freshwater forma-

tions with an intervening formation of marine origin. The strata of the upper freshwater were found to vary from about 30 to 40 feet in thickness, those of the lower from 13 to 28 feet, the latter being sometimes lower than the present level of the Mediterranean.

The marine beds which are interposed are from 10 to 11 feet thick.

The freshwater strata are composed of numerous alternating calcareous and argillaceous marls, and compact limestones; and their organic remains consist of a few bones of land quadrupeds much decayed, a variety of freshwater and terrestrial shells, the latter in the greatest abundance; the shells differing in species but not in genera from the present inhabitants of the same country; and lastly, some traces of vegetables, chiefly reeds.

The marine formations contain *ostrea*, *cerithæa*, &c. A complete list is added of the organic remains; and from the state of preservation in which the freshwater shells are found, M. Marcel de Serres infers that they lived and were deposited where they are now found; and from the resemblance of those occurring in the upper and lower freshwater beds, he concludes that the periods at which these two formations were deposited were not very remote from each other.

The author considers all these formations to be more recent than the *calcaire grossière*, and ascribes the alternations of marine and freshwater strata to a return of the sea, such a supposition being rendered the more probable by the neighbourhood of the Mediterranean, where similar returns are still known to take place.

*Feb. 4.*—This day being the Anniversary of the Society, the following gentlemen were chosen as the Officers and Council for the year ensuing.

*President.*—Rev. W. Buckland, FRS. Prof. Geol. and Min. Oxford.

*Vice-Presidents.*—Sir A. Crichton, MD. FR. and LS. Hon. Memb. Imp. Acad. St. Petersburg; W. H. Fitton, MD. FRS.; C. Stokes, Esq. FRA. and LS.; and H. Warburton, Esq. FRS.

*Secretaries.*—C. Lyell, Esq. FLS.; G. P. Scrope, Esq.; T. Webster, Esq.

*Foreign Secretary.*—H. Heuland, Esq.

*Treasurer.*—J. Taylor, Esq.

*Council.*—Hon. H. G. Bennet, MP. FRS. and HS.; R. Bright, MD. FRS.; Sir H. Bunbury, Bart.; H. Burton, Esq.; W. Clift, Esq. FRS.; H. T. Colebrooke, Esq. FRSL. and E. FL. and Asiat. S.; G. B. Greenough, Esq. FR. and LS.; T. Horsfield, MD. FLS.; G. Mantell, Esq. FLS.; Hugh Duke of Northumberland, KG. FHS.; W. H. Pepys, Esq. FRS. LS. and HS.; and J. Vetch, MD.

## ARTICLE X.

## SCIENTIFIC NOTICES.

## CHEMISTRY.

1. *Cold produced by the Combination of Metals.*

The elevation of temperature and even brilliant ignition which take place at the instant of the combination of certain metals, as of potassium or sodium with mercury, of tin or zinc with platinum, &c. have been long familiarly known; but chemists were not, we believe, acquainted with the existence of any analogous instances, in which the combination of metals is followed by the production of *cold*. Sir H. Davy, indeed, ascertained that the solid amalgams of bismuth and of lead become *fluid* upon being mixed; but he does not appear to have examined whether or not any *depression* of temperature results from this sudden liquefaction. Some curious examples of this latter description have been lately noticed by Döbereiner.

According to him, the fusible metal is a compound of 1 atom of lead ( $= 103.5$ ), 1 atom of tin ( $= 59$ ), and 2 atoms of bismuth ( $= 2 + 71$ ); or it consists of 1 atom of the atomic combination of bismuth and lead, united to 1 atom of the atomic combination of bismuth and tin ( $\text{Bi Pb} + \text{Bi Sn}$ ); and it becomes fluid when exposed to a temperature of  $210^\circ$ . The melting points of these alloys of bismuth and lead and of bismuth and tin in a separate state, are respectively between  $325^\circ$  and  $335^\circ$ , and between  $268^\circ$  and  $280^\circ$ . If 118 grains of filings of tin, 207 grains of filings of lead, and 284 grains of pulverised bismuth (the constituents of fusible metal), be incorporated in a dish of calendered paper with 1616 grains of mercury, the temperature instantly sinks from  $65^\circ$  to  $14^\circ$ . He is of opinion that it would even fall so low as the freezing point of mercury, were this experiment performed in temperatures somewhat under  $32^\circ$ .

In like manner, when 816 grains of the amalgam of lead (composed of 404 mercury + 412 lead  $= \text{Pb Hg}$ ), were mixed, in a temperature of  $68^\circ$  with 688 grains of the amalgam of bismuth (composed of 404 mercury + 284 bismuth  $= \text{Bi Hg}$ ), the temperature suddenly fell to  $30^\circ$ , and by the addition of 808 grains of mercury (also at  $68^\circ$ ), it became so low as  $17^\circ$ : the total depression amounting to no less than 51 degrees.—(Schweigger's *Neues Journal der Chemie und Physik*, xii. 182.)

Nothing can be a more decisive proof that metallic alloys are true chemical combinations, than the foregoing experiments of Döbereiner; and it is to be regretted, therefore, that our

nomenclature in its present state scarcely affords a concise, and, at the same time, a definite appellation for this class of compounds. The German chemists express them without difficulty, and on precisely the same principles by which they express the compounds of metals with sulphur, &c. We have already no hesitation in employing the terms seleniuret, arseniuret, &c. Might we not extend the usage somewhat further, and designate metallic alloys in general by the termination *uret*: thus, potassiumuret, plumburet, stannuret, &c.?

### 2. Conversion of Gallic Acid into Ulmin by Oxygen Gas.

According to Döbereiner, when a solution of gallic acid in liquid ammonia is placed in contact with oxygen gas, it gradually absorbs as much of the latter as is requisite to convert the whole of its hydrogen into water. 100 parts of the acid absorb 38.09 parts of oxygen. In the ordinary atmospheric temperatures, the absorption is complete at the end of from 18 to 24 hours. While this change is going on, the solution becomes intensely brown coloured and opaque, and on the addition of muriatic acid, it lets fall a *pale brown coloured flocculent substance, which possesses all the characters of Ulmin*. From this experiment Döbereiner considers it probable that ulmin consists of an atom of oxide of carbon ( $\equiv 12 \text{ carbon} + 8 \text{ oxygen}$ ) in combination with an atom of water ( $= 1 \text{ hydrogen} + 8 \text{ oxygen}$ ): it is certain, at least, that if the details of the experiment be accurate, the constituents of the gallic acid (according to Berzelius's analysis) taken in conjunction with the absorbed oxygen, are resolvable into this simple ratio of atoms.

Gallic acid prepared by Scheele's process, even after having been crystallized from absolute alcohol, absorbs considerably less oxygen than the sublimed acid, and it appears, therefore, to be still contaminated with tannin, or with some other foreign admixture.—(Pneumatische Chemie, Vierter Theil.)

### 3. Formic Acid.—Formic Ether.

Formic acid may be easily analyzed by mixing it either uncombined, or in the state of a neutral salt, with from 6 to 10 times its weight of concentrated sulphuric acid: it is instantly resolved with effervescence into 24.30 parts of water, and 75.70 parts of carbonic oxide gas.

Hence it may be regarded as constituted of 1 volume of the vapour of water + 2 volumes of carbonic oxide gas. This supposition is strengthened by the facility with which it is converted into carbonic acid and water by the action of the oxides of silver and of mercury.

When formic ether stands in contact with water, it is gradually decomposed into formic acid and alcohol: during the decompo-

sition, no elastic fluid is either absorbed or disengaged. To ascertain the proportion of formic acid which is set at liberty during the decomposition of a given quantity of the ether, four grains (previously rectified by distillation of chloride of calcium) were let up into a solution of bicarbonate of potash standing in a glass tube over mercury. The disengagement of gas commenced after a few minutes, and lasted for about three days: it was most copious when the light of the sun was strongest. The gas evolved measured 3·893 (German) cubic inches, = 2·103 grains. This is equivalent to 1·768 gr. of formic acid. Now the atomic weight of formic acid is 37, and that of alcohol is 46; and  $1·768 : 2·232 (4 - 1·768) :: 37 : 46·8$ . Consequently formic ether may be regarded as constituted of an atom of formic acid in combination with an atom of alcohol.

Formic ether does not become acid in alcohol slightly diluted with water, and behaves, therefore, in an analogous manner with many of the compounds of chlorine with the acid metals (tellurium, arsenic, antimony, &c.) which are dissolved by alcohol without undergoing any alteration, but are decomposed into muriatic acid and metallic oxides when mixed with water.—(Ibid.)

#### MINERALOGY.

##### 4. *Table of the Specific Gravities of several Minerals.*

The specific gravities of the following substances, which are disposed nearly in the order of the system of Prof. Mohs, were taken by William Haidinger, Esq. FRSE.

##### ORDER I.—HALOIDE.

1. Gypsum, a perfectly white transparent crystal from Oxford .....	2·310
2. Anhydrite, a rectangular four-sided prism, obtained by cleavage, grey, semitransparent, from Hall, Tyrol ..	2·899
3. Alumstone, the crystallised variety on the surface exposed in the drusy cavities, from Tolfa ..	2·694
4. The compact part of the same specimens. ....	2·671
5. Kryolite, the white cleavable variety. ....	2·963
6. Apatite, massive, asparagus-green, transparent, from Salzburg. ....	3·180
7. Apatite, asparagus-green crystals, from Cabo de Gata ..	3·225
8. Fluor, combinations of the hexahedron and octahedron, dark violet blue from St. Gallen, Styria ..	3·140
9. Fluor, an octahedron obtained by cleavage, of a greenish blue colour, from the Hartz .....	3·163
10. Fluor, twin crystals, pale violet blue by reflected light, yellowish white by transmitted light, Alston ....	3·177
11. Fluor, an octahedron obtained by cleavage, pale violet blue, Alston .....	3·178

12. Arragonite, yellowish white, perfectly transparent crystals, from Bohemia ..... 2·931
13. Calcareous spar, a brown cleavable variety. .... 2·715
14. Calcareous spar, another brown cleavable variety, but presenting curved faces of cleavage ..... 2·721
15. Calcareous spar, crystallised in the form of  $(P + 1) \frac{1}{2}$   $R + \infty$ , white, semitransparent, from Alston, Cumberland..... 2·721
16. Calcareous spar, yellowish grey, small individuals aggregated in a granular composition ..... 2·727
17. Calcareous spar, individuals of a columnar composition, honey-yellow, semi-transparent. .... 2·731
18. Calcareous spar, in large cleavable individuals, of a reddish brown colour, owing to the admixture of oxide of iron. This variety was sent from Paris to the collection at Gratz, as *chaux carbonatée ferrigère*. .... 2·778
19. Calcareous spar, white translucent cleavable masses, engaged in the hydrate of magnesia from Unst (see Order 5, 20) ..... 2·647
20. Calcareous spar, crystals of the form of the fundamental rhombohedron, associated with small crystals of adularia, epidote, and chlorite, from Dauphiné. 2·508
21. Macrotypons lime-haloide, brown spar, greyish white crystals of the form of R, perfectly cleavable in pretty even faces, lustre almost pearly; is found in Gollinggraben in Salzburg, in fissures of a limestone rock. .... 2·842
22. Brown spar, greyish white, easily cleavable, affording brilliant planes, Frieberg. .... 2·861
23. Brown spar, reddish white crystals of the form  $R_1(P)_1$ , from the Himmelfarth mine near Frieberg .. 2·870
24. Rhomb spar, greyish white, cleavable, from a bed of octahedral iron ore, where it is associated with amphibole, &c. from Presnitz, Bohemia. .... 2·859
25. Dolomite, white granular composition, forming the mass in which tremolite is imbedded, from St. Gothard. .... 2·859
26. Rhomb spar, yellowish white, perfectly cleavable. .. 2·878
27. Ankerite, yellowish white cleavable masses from Eisenerz, Stiria. .... 3·000
28. Ankerite, in granular compositions consisting of small individuals of a grey colour, from the laiding mountain in Stiria. .... 3·049
29. Ankerite, a greyish white granular variety, from the valley of Rötz, in Stiria ..... 3·084
30. Ankerite, large cleavable masses, of a cream yellow colour, from Golrath, Stiria. .... 089

- 31.\* **Brunnerite**, a clove brown, perfectly cleavable variety, forming imbedded crystals, from the Tyrol. .... 3·001
32. **Wavellite**, globular shapes of a dirty asparagus green colour, from Barnstaple, Devonshire. .... 2·337

## ORDER II.—BARYTE.

1. **Red manganese**, a massive variety, compound parallel to the planes of  $R = \infty$ , like slate spar, from Beschertglück mine, near Frieberg. .... 3·428
2. **Sparry iron**, crystals from the Pfaffenbergmine, near Harzgerode, in the Hartz. .... 3·829
3. **Prismatic zinc baryte**, yellowish white, semitransparent crystals, from Rossegg, Carinthia. .... 3·380
4. **Rhombohedral zinc baryte**, honey yellow crystals, in the shape of rough six-sided pyramids, from Altenberg, near Aix-la-Chapelle. .... 4·441
5. **Tungsten**, a fragment of a yellowish white translucent crystal, from Schlaggenwald, Bohemia. .... 6·076
6. **Strontianite**, delicate white crystals, aggregated to globular groupes, from Bräunsdorf, Saxony. .... 3·605
7. **Celestine**, fragment of a cleavable white translucent mass, engaged in trap, from the Tyrol. .... 3·858
8. **Witherite**, a cleavable variety; yellowish white, and semitransparent, from Anglesark, Lancashire. .... 4·30
9. **Heavy spar**, very thin tabular bluish white semitransparent crystal, of the form primitive of Haidy, from Kreinitz, Hungary. .... 4·412
10. **Heavy spar**, a number of small transparent columnar crystals, of a white colour, from the Hartz. .... 4·415
11. **Heavy spar**, cleavable, very pale yellowish grey, and translucent, from Marienberg, Saxony. .... 4·415
12. **Heavy spar**, the variety called prismatic heavy spar by Werner, pale yellow, transparent crystals, very perfectly formed, and imbedded in a large translucent crystal of straight lamellar heavy spar. .... 4·426
13. **Heavy spar**, prisms obtained by cleavage, white, and semitransparent. .... 4·430
14. **Heavy spar**, yellowish translucent crystals, from Kremnitz. .... 4·430
15. **Heavy spar**, similar crystals from Beschertglück, Frieberg. .... 4·445
16. **Heavy spar**, white, semitransparent crystals, from Beschertglück. .... 4·446

\* This is the brachytypous lime-haloide of Mohs, the carbonate of iron and manganese of Brooke.

17. Heavy spar, small blue transparent tabular crystals, from Offenbanya, Transylvania ..... 4·473
18. Heavy spar, a white transparent crystal, from Dufton, Westmoreland ..... 4·480
19. Heavy spar, in white faintly translucent columnar compositions, commonly called columnar heavy spar, from the abandoned mine of Lorenz Gegendmen, Frieberg ..... 4·488
20. Heavy spar, a single columnar crystal, pale smoke grey, translucent, from Hiskow, near Nissburg, Bohemia, where it occurs with copper pyrites, blende, and calcareous spar, in a kind of septaria. 4·493
21. Heavy spar, pale yellow transparent columnar crystals, from Przibram, Bohemia. .... 4·210
22. Heavy spar, prisms obtained by cleavage from wax yellow, translucent, tabular crystals, from Bleiberg, Carinthia ..... 4·679
23. Di-prismatic lead baryte (carbonate of lead) columnar compositions, perfectly white, almost opaque, from the Hartz ..... 6·339
24. Di-prismatic lead baryte, similar composition, but of a yellowish colour, superficially almost brown, from the Hartz ..... 6·417
25. Di-prismatic lead baryte, greyish white, easily cleavable crystals, from Bleiberg, Carinthia ..... 6·461
26. Di-prismatic lead baryte, fragment of a white strongly translucent crystal, from Leadhills ..... 6·465
27. Rhombohedral lead baryte (phosphate of lead), a single green crystal, from Zschopau, Saxony. .... 7·098
28. Arseniate of lead, bright yellow crystals, from Johanngeorgenstadt, Saxony ..... 7·212
29. Hemiprismatic lead baryte (chromate of lead), several isolated crystals from Siberia ..... 6·004
30. Pyramidal lead baryte (molybdate of lead), longish deep wax yellow crystals, from Bleiberg, Carinthia. .... 6·698
31. Pyramidal lead baryte, fragments of an orange yellow, perfect crystal, from Annaberg, Austria. .... 6·760
32. Prismatic lead baryte (sulphate of lead), broad, deeply striated crystals, of a white colour, and faint translucency, from Leadhills ..... 6·228
33. Prismatic lead baryte, a white translucent tabular crystal, from Leadhills ..... 6·298
35. Prismatic lead baryte, fragments of a large semi-transparent crystal, from Leadhills ..... 6·309
36. Axotomous lead baryte (sulphato-tri-carbonate of lead), the acute crystals commonly called rhom-

- bohedrons, of a dark yellowish grey colour, and translucent, from Leadhills. . . . . 6.266
37. Axotomous lead baryte, the six-sided laminae, of a pale yellowish white colour, semitransparent, from Leadhills . . . . . 6.364
38. White antimony, transparent crystals, about 1''' in diameter, yellowish white, from Braunsdorf, Saxony. . . . . 5.566

## ORDER III.—KERATE.

- 1. Horn-ore, a very pure, greyish white, translucent variety, compounded of granular individuals, from Peru . . . . . 5.552

## ORDER IV.—MALACHITE.

1. Copper green, massive, fracture conchoidal; colour, dark verdigris green, translucent, from Siberia. . . 2.031
2. Copper green, thin botryoidal coats upon compact brown iron ore, pale green, faintly translucent, Bannat . . . . . 2.206
3. Prismatic lirocone malachite (lenticular copper) sky-blue crystals, from Cornwall . . . . . 2.926
4. Prismatic azure malachite (blue carbonate of copper), fragments of very pure crystals, from Chessy. . . . 3.831
5. Malachite, a cleavable dark green variety, from Chessy . . . . . 4.008
6. Malachite, a fibrous dark green variety, from Siberia . . . . . 3.802
7. Malachite, perfectly compact, of a pale green colour, opaque, from Schwartz, Tyrol . . . . . 3.670
8. Prismatic habroneme malachite (phosphate of copper), dark green crystalline coat, from Rheinbreitbach, on the Rhine. . . . . 4.206
9. The radiated acicular olivenite of Jameson, oblique prismatic arseniate of Phillips, globular shapes of a dark blue colour, a little greenish, translucent. . . 4.192
10. Scorodite, pale green, semitransparent crystals, from Stamm Asser am Graul, Saxony. . . . . 3.162

## ORDER V.—MICA.

1. Vivianite (phosphate of iron), fragments of transparent crystals from St. Agnes, Cornwall . . . . . 2.661
2. Cobalt-bloom (arseniate of cobalt), red acicular crystals, perfectly cleavable, from Schneeberg, Saxony . . . . . 2.946
3. Cobalt-bloom, showing red and green colours in the same crystals, from Gotthold-Stollh, near Platten, Bohemia . . . . . 3.033
4. Talc, apple green laminae, from the Greiner mountain in Salzburg . . . . . 2.744

5. Chlorite, loose scaly particles of a dark green colour, earthy chlorite of Werner . . . . . 2·706
6. Chlorite, massive, composed of large granular individuals, dark green, from the Rothen Kopf mountain in Salzburg. . . . . 2·713
7. Chlorite, of the same kind, only the individuals smaller. . . . . 2·729
8. Chlorite, a similar variety, consisting of still smaller individuals. . . . . 2·731
9. Chlorite in large laminae, and most perfectly cleavable, more translucent, from the same locality . . . 2·775
10. Chlorite, liver brown rhombic prisms, imbedded in compact green chlorite, from the same locality. . . 2·781
11. Chlorite, composition almost impalpable, and fracture slaty, of a dark mountain green colour . . . . . 2·799  
     This variety contains minute crystals of rutile.
12. Green earth, a compact, celandine green variety, from Monte Baldo, near Verona . . . . . 2·834  
     On account of the difficulty of obtaining it free from mechanical admixtures, this specific gravity is, perhaps, not quite exact.
13. Mica, perfectly cleavable individuals, engaged in granite, showing iridescent fissures parallel to the laminae, colour oil green perpendicular to the axis, more brown parallel to it, from the Schwamberg Alps in Stiria. . . . . 2·883  
     It has two axes of double refraction, like the white mica from Siberia.
14. Mica, perfectly black, in a granular composition, exhibiting a tendency to slaty structure, from the district of Pinzgan, in Salzburg . . . . . 2·911
15. Mica, silver white crystals, from Zinnwald, Saxony . . 2·945
16. Mica, greenish black, in large perfectly cleavable individuals, Siberia . . . . . 2·949
17. Lepidolite, peach blossom red, compound of granular individuals, from Rosena, Moravia . . . . . 2·831
18. Another specimen of the same. . . . . 2·833
19. Pearl mica, perfectly cleavable, reddish white crystals 3·022
20. Hydrate of magnesia, white laminae, perfectly cleavable and translucent, from Unst . . . . . 2·350

(Edinburgh Journal of Science.)

## ARTICLE XI.

## NEW SCIENTIFIC BOOKS.

## PREPARING FOR PUBLICATION.

Dr. Olinthus Gregory has in the press a work on Pure and Mixed Mathematics, with their Practical Applications, intended especially for the Use of Mechanics and Civil Engineers. In 1 volume, 8vo. illustrated with numerous Diagrams and Wood-cuts.

## JUST PUBLISHED.

On the Safety Lamp, for preventing Explosions in Mines, Houses, &c. By Sir Humphry Davy, Bart. Pres. Royal Society. With Additions. 8vo. 7s. 6d.

Forsyth's Medical Pocket Book. 6s.

Keating's Travels to St. Peter's River, &c. 1l. 8s.

An Attempt to establish the First Principles of Chemistry by Experiment. By T. Thomson, MD. &c. 2 vols. 8vo. 1l. 10s.

Nicholson's Operative Mechanic. 100 Plates. 8vo. 1l. 10s.

## ARTICLE XII.

## NEW PATENTS.

Chevalier Joseph de Mettemberg, Foley-place, Mary-le-bone, physician, for a vegetable mercurial and spirituous preparation called *Quintessence Aulepsorique*, and also a particular method of employing the same by absorption as a specific and cosmetic.—Feb. 26.

J. Masterman, Old Broad-street, for an improved method of corking bottles.—March 5.

A. H. Chambers, and E. Chambers, Stratford-place, Mary-le-bone, and C. Jearrard, Adam-street, Manchester-square, for a new filtering apparatus.—March 5.

W. Halley, Holland-street, Blackfriars-road, iron-founder and blowing-machine maker, for improvements in forges, and on bellows or apparatus to be used therewith or separate.—March 5.

R. Winch, Steward's Buildings, Battersea Fields, engineer, for improvements in rotary pumps for raising water, &c.—March 5.

W. H. James, Cobourg-place, Winson Green, near Birmingham, engineer, for improvements on rail-ways, and carriages to be employed thereon.—March 5.

W. Hirst and J. Wood, Leeds, for improvements in cleaning, milling, or fulling cloth.—March 5.

J. L. Bond, Newman-street, Mary-le-bone, architect, and J. Turner, Well-street, Mary-le-bone, builder, for improvements in the construction of windows, casements, folding sashes, and doors, by means of which the same are hung and hinged in a manner adapted more effectually to exclude rain and wind, and to afford a free circulation of air.—March 9.

T. Hancock, Goswel Mews, St. Luke's, patent cork-manufacturer, for a new manufacture which may be used as a substitute for leather and otherwise.—March 15.

T. Hancock, Goswel Mews, for improvements in making ships' bottoms, vessels and utensils of different descriptions and various manufactures, and porous or fibrous substances, impervious to air and water, and for coating and protecting the furnaces of different metallic and other bodies.—March 15.

T. Hancock, Goswel Mews, for improvements in the process of making or manufacturing ropes or cordage and other articles from hemp, flax, &c.—March 15.

J. Colling, Lambeth, engineer, for improvements on springs and other apparatus used for closing doors.—March 15.

R. B. Bate, Poultry, optician, for his improvement on the frames of eye-glasses.—March 15.

H. Nunn, and G. Freeman, Blackfriars-road, lace-manufacturers, for improvements in machinery for making that sort of lace commonly known by the name of bobbin net.—March 15.

S. Brown, Saville-row, Middlesex, for his apparatus for giving motion to vessels employed in inland navigation.—March 15.

J. Barlow, New Road, Middlesex, sugar-refiner, for his process for bleaching and clarifying and improving the quality and colour of sugars known by the name of bastard and piece sugars.—March 15.

W. Grisinthwaite, King's Place, Nottingham, for his improvement in air-engines.—March 15.

R. Whitechurch and J. Whitechurch, Star-yard, Cary-street, Middlesex, for an improvement on hinges for doors, &c. which will enable the doors, &c. to be opened on the right and left (changing the hinges), and with or without a rising hinge.—March 17.

M. Cosnahan, Isle of Man, for a new apparatus for ascertaining the way and leeway of ships, also applicable to other useful purposes.—March 17.

R. Hicks, Conduit-street, surgeon, for an improved bath.—March 22.

F. Ronalds, Croydon, for a new tracing apparatus to facilitate the drawing from nature.—March 23.

R. Wilty, Kingston-upon-Hull, civil engineer, for an improvement in the method of lighting by gas, by reducing the expense thereof.—March 25.

J. Martin Hanchelt, Crescent-place, Blackfriars, and J. Delvalle, Whitecross-street, Middlesex, for an improvement in looms for making cloths, silks, and different kinds of woollen stuffs, of various breadths.—March 25.

J. Manton, Hanover-square, gun-maker, for an improvement in shot.—March 25.

J. G. Ulrich, Bucklersbury, London, chronometer-maker, for improvements on chronometers.—March 25.

A. Jennins and J. Belteridge, Birmingham, japanners, for improvements in the method of preparing and working pearl-shell into various forms and devices, for the purposes of applying it to ornamental uses in the manufacture of japan ware and of other articles.—March 29.

## ARTICLE XIII.

## METEOROLOGICAL TABLE.

1825.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.
		Max.	Min.	Max.	Min.		
3d Mon.							
March 1	W	29.92	29.39	45	34	—	
2	N W	29.60	29.56	48	32	—	48
3	N W	29.80	29.60	45	32	—	
4	N W	30.39	29.80	42	28	—	
5	N	30.43	30.33	42	30	—	
6	S	30.33	29.89	45	32	—	
7	SSE	30.39	29.89	47	32	—	
8	N W	30.42	30.39	50	39	—	11
9	S W	30.42	30.41	50	45	—	—
10	S W	30.41	30.32	53	48	—	10
11	S W	30.30	30.26	52	37	—	02
12	N	30.30	30.29	50	37	—	—
13	S W	30.29	30.26	46	30	—	—
14	E	30.33	30.26	38	29	—	05
15	E	30.41	30.33	38	27	.55	
16	N	30.61	30.41	41	21	—	—
17	S E	30.70	30.61	40	25	—	
18	ESE	30.73	30.70	45	21	—	
19	E	30.77	30.73	45	24	—	
20	N E	30.78	30.71	53	26	—	
21	N E	30.71	30.49	54	36	—	
22	N E	30.49	30.34	40	34	—	
23	E	30.34	30.26	48	28	—	
24	N E	30.26	30.04	52	30	—	
25	E	30.16	30.04	52	34	—	
26	N E	30.23	30.16	50	32	.43	
27	E	30.23	30.22	58	30	—	
28	N E	30.22	30.18	52	35	—	
29	E	30.18	30.14	48	38	—	
30	E	30.43	30.14	48	34	—	
31	E	30.61	30.43	52	33	.32	
		30.78	29.36	58	21	.38	.76

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

## REMARKS.

*Third Month.*—1. Fine. 2. Fine day: rainy night. 3. Fine. 4. Some hail, p. m.: cold wind all day. 5, 6. Fine. 7. Cloudy. 8. Fine. 9. Cloudy. 10. Gloomy. 11. Rainy morning. 12. Cloudy. 13. Gloomy. 14. Some snow this morning. 15. Fine. 16. Some snow at intervals during the day. 17—21. Fine. 22. Cloudy. 23—26. Fine. 27. Fine: a lunar corona at night. 28. White frost: cloudy. 29—31. Fine.

## RESULTS.

Winds: N, 3; NE, 6; E, 9; SE, 3; S, 1; SW, 4; W, 1; NW, 4.

Barometer: Mean height

For the month. . . . . 30·121 inches.

\* For the lunar period, ending the 11th. . . . . 30·266

For 13 days, ending the 4th (moon north) . . . . . 30·140

For 14 days, ending the 18th (moon south) . . . . . 30·372

Thermometer: Mean height

For the month. . . . . 39·709°

For the lunar period, ending the 11th . . . . . 39·714

For 29 days, the sun in Pisces. . . . . 40·293

Evaporation . . . . . 1·30 in.

Rain. . . . . 0·76

And by a second gauge . . . . . 0·80

# ANNALS OF PHILOSOPHY.

JUNE, 1825.

## ARTICLE I.

*Additional Experiments and Remarks on Light and Heat.*  
By Baden Powell, MA. FRS.

(1.) IN my last communication I mentioned several experiments I had tried, by way of varying the principal ones, on which my conclusion relative to the existence of two distinct species of heat, in the emanation from luminous hot bodies, depended. I now beg leave to lay before the readers of the *Annals* one or two other experiments having the same object in view. These were made with a large differential thermometer having the bulbs differently coated, as hereafter expressed. It was placed with the bulbs exactly in a line from the source of heat, each being alternately nearest; and each being tried with and without the intervention of a glass screen. If the effect were due to one simple radiating agent, the ratio of the effects on the smooth black and the absorptive white with the screen, ought to be the same as without: the following results however indicate a considerable difference; the divisions are not Lesliean degrees. The first column describes which bulb was nearest the source of heat. They were nearly three inches asunder, and at about two from the flame, and six from the hot iron.

Flame of a candle. Effect in 1 min.

	Screened.	Exposed.
Indian ink . . . . .	2 . . . . .	4
Thin brown silk . . . . .	0 . . . . .	5

Argand lamp. No chimney.

Indian ink . . . . .	3 . . . . .	4
Thin brown silk . . . . .	1 . . . . .	3

Incandescent iron. Effect in 30 seconds.

Indian ink . . . . .	2 . . . . .	4
Thin brown silk . . . . .	1 . . . . .	5

Argand lamp. Instrument stationary at,  
Screened. • Exposed.

Indian ink ..... 9 ..... 12  
Silk. .... 1 ..... 8

(2.) In an account given by Dr. Wollaston of his celebrated researches on the chemical effects of solar light (Nicholson's Journal, 8vo. vol. viii. p. 293), after showing that the green colour which is communicated to guaiacum by the violet rays, is removed on exposure to the red; and that the same effect is produced by the application of hot metal by conduction, Dr. W. makes the following remark:—

“The last experiment may possibly appear to have been unnecessary; but until it is explained why the heat that accompanies the sun's rays penetrates the substance of transparent or semitransparent bodies, while the radiant heat from a fire has scarcely power to enter even the most transparent, but principally scorches the surface, and is thence slowly conducted into the interior parts: no degree of caution upon a subject so imperfectly understood should be deemed superfluous.” P. 297.

I have quoted this instance of the distinguished author's judicious and well known caution as a contrast to many passages which might be found in the writings of some of our most eminent philosophers. The confessedly imperfect state of our knowledge upon these subjects must show the importance of every step we can with caution and certainty take towards the elucidation of them. Instances are not wanting in the productions of very distinguished men which exhibit a great vagueness and obscurity of ideas on these points, an evil which has probably been much increased by the adoption of theoretical views respecting “calorific rays,” “luminous caloric,” “non-luminous light,” &c.

(3.) As bearing upon an important part of the subject, I am led to notice the following theoretical view of the mode in which a glass screen acts, on the supposition of an actual radiation of heat through it, given by Biot: (*Traité de Phys.* iv. 636)..... “Si l'air à travers lequel la transmission s'opère, *absorbait* une portion sensible de ce calorique, et lui *laissait* un *passage* d'autant plus libre, qu'il émanerait d'un corps plus chaud. On verra tout-à-l'heure que cet effet a lieu pour les lames de verre, quand on les interpose dans le courant calorifique;” ..... &c.

If this were the case, it is difficult to conceive how a thin plate of glass should cause a less diminution of effect than a thick piece, for the thin glass would certainly abstract much less heat, and the more heated the glass became, the less heat it would absorb, and therefore transmit less, both which we know are contrary to the fact.

(4.) In reference to the history of investigations respecting

light and heat, it may not be irrelevant to remark, that in a late publication, the originality of Prof. Leslie's theory seems to be brought into question. His "Inquiry" was published in 1804, and in his preface he states, "that the leading facts presented themselves in the spring of 1801." In the Life and Remains of Dr. Clarke (4to. 491), will be found a letter from that philosopher to Mr. Malthus, dated from Egypt, Sept. 9, 1801, in which he describes some discussion he had had with the savans in that country, in which he had proposed and maintained the theory that light and caloric are identical, but only existing in different states.

(5.) In reference to the history of the investigation, the following notice may also not be uninteresting. Dr. Fordyce in a paper on the light produced by inflammation (Phil. Trans. 1776, p. 504), makes a distinction between the light produced from the inflammation or ignition of bodies, and that derived from their decomposition. He proves that the latter is the case with phosphorus. This light he maintains to be totally independent of heat; but there is nothing in his paper which can amount to a proof of this. He considers the blue part of flame to be produced by decomposition, not by ignition or inflammation, which is subsequently effected in the other parts of the flame. He has pointed out the fact that light may be evolved from some substances, as sulphur, by the application of a less degree of heat than that requisite to evolve it from the other ingredients of gunpowder, though he considers this to arise from the former process being not a true ignition, whilst for all real ignition one particular temperature is required.

These views must be considered curious; and were perhaps the first steps towards the correct theory of inflammation, since so fully established by Sir H. Davy, &c.

(6.) In the *Annals* for May, 1824, Art. 5, p. 352, the lovers of theory will find a view of the generation and nature of light, as deduced by Mr. Herapath from his ingenious and recondite theory of evaporation, heat, &c. which exactly accords with that here deduced upon principles entirely different. That philosopher I believe in some other parts of his speculations opposes the commonly received views of latent heat, and consequently could not consistently bring light under the dominion of these laws; but it would seem that he regards light as in every respect analogous to vapour; and thus if we admit the doctrine of latent heat in the one case, the way is so far smoothed, even by an opponent, for its admission in the other.

(7.) The difficulty which I before adverted to as attaching to the theory of the conversion of heat into light, viz. that only a part of the heat undergoes this change, will, on the theory here advanced, no longer exist. Any given body has only a definite quantity of light in combination, and only a definite quantity of heat is requisite to liberate it; the remaining portion therefore

continues to act its natural part without undergoing any alteration.

(8.) (Brande's Chemistry, i. 297, 2d Edit.; Davy's Elements, p. 215.) "Newton has put the query whether light and common matter are not convertible into each other? and if we consider sensible heat in bodies to depend upon vibrations of their particles, a certain intensity of vibrations may send off particles into free space; and particles moving rapidly in right lines may, in losing their own motion, communicate a vibrating motion to the particles of terrestrial bodies."

Without any hypothesis as to the nature of heat, it is obvious that the principle above adopted will readily explain the convertibility of common matter into light, or at least of light existing in a state of material combination, into light in a free radiant state. Whether the light consist of particles of the bodies from which it is generated combined with latent heat, or of peculiar particles entirely of a separate species, at first existing in combination with solid matter, and then liberated and brought into the state of luminous elastic fluid by the agency of latent heat, we have probably no means of deciding. It is possible that light may be formed from certain particles of the body which are made to assume a radiant state in a way analogous to the formation of vapour; but the opinion that the light is a peculiar substance in combination with the body from which it is extricated seems to be the more probable one from the fact of the absorption of light by various bodies.

(Phil. Trans. 1817, Part I. p. 75.) Sir H. Davy has concluded that the luminosity of flames is greater in those cases where solid particles are volatilized and ignited. This is exactly conformable to the principle here advanced. Solid particles ought to be more readily convertible into light, *ceteris paribus*, than those of elastic fluids. They have less capacity for heat; therefore of the heat communicated to them, a larger share can go to the evolution of light, and a greater quantity of light is condensed in the same space.

Hence it would follow also that the less solid the product the less luminous would the flame be, and therefore the less heat would be employed in producing light, and consequently the more in raising temperature.

It may arise from the peculiar constitution of bodies, that their heat may in some cases be more employed in producing light, and in others more in increasing temperature or radiating heat: thus the ratio of heat really produced in flames may be very different from what appears.

I have alluded to the production of light from some particular sources only. There are, as is well known, several others from which it is generated, and to these must the examination be extended before we can, strictly speaking, consider the conclusion as

universally true. Upon these topics I shall probably offer some remarks at a future opportunity.

(9.) The following remarks of M. Biot in reference to the colour of flames are interesting in connexion with the present inquiry.

(Biot, *Traité de Physique*, vol. iv. 617.) “ Enfin, puisque, selon les observations de De la Roche, le calorique obscur, émané d'un corps que l'on échauffe graduellement, approche aussi graduellement des conditions et des propriétés que possède le calorique lumineux, on conçoit que, lorsque l'émanation commence à devenir visible, elle doit être d'abord analogue à la partie la moins calorifique du spectre, qui se trouve à l'extrémité violette. Aussi observe-t-on que toutes les flammes, lorsqu'elles commencent à naître, sont d'abord violettes ou bleues, et n'atteignent la blancheur que lorsqu'elles ont acquis un plus haut degré d'intensité.

*Note.* “ Cette progression de teintes a même lieu pour la lumière que l'étincelle électrique exerce dans l'air. Je m'en suis assuré en tirant ces étincelles à diverses distances, entre une pointe mousse et une sphère métalliques : disposition qui permettait d'obtenir un jet continu, dont on modérait à volonté l'intensité par l'éloignement.”

According to the theory I have proposed, we may make the following remark upon the points just specified.

Violet rays have less latent heat than red ; and if two flames be equal in other respects, but one of a red, and the other of a blue colour, the temperature of luminosity for red rays will be less than that for blue rays. Less of the heat will be occupied in converting the matter into blue than into red light. Or again when the chemical action is less, less heat can be afforded for the formation of light, and blue rays will be formed. When more heat is generated by an increased action, yellow, red, &c. rays may result.

(10.) The incandescence of metals may clearly be regarded as the lowest stage of combustion : a combination with oxygen is evidently going on, which, in the instance of iron at a white heat, becomes very perceptible, from the oxide breaking off in scales. Thus taking the whole range of the phenomenon from a dull red heat up to the most intense combustion in oxygen gas, we may observe the metal giving out light which passes through all the tints successively, from the deepest and almost invisible red to nearly perfect whiteness.

The observation of the process of combustion in some other cases, as in flames, exhibits a different succession of appearances. Here at the lowest or most imperfect stage, the rays given off are violet or blue, and these gradually pass with the increasing completeness of combustion into whiteness.

Thus we have two classes of the phenomenon of combustion,

in which the effects seem to pass through two different orders of changes, but both receive their completion at the same point; and this difference would seem to depend on some peculiar difference in the nature of the sources from which the light emanates.

If we attempt to enumerate the various sources of light which comprise the two classes described, the only *distinction* which, as far as I know, we can fix upon, is that of the one class consisting of metals and carbon; the other of sulphur, phosphorus, and hydrogen.

(11.) To Sir H. Davy we are indebted for the most important acquisitions which have been made to our knowledge of the nature of flame. The observation of the different peculiar temperatures required in order to produce luminosity in different species of inflammable gas, and the constant maintenance of that temperature, while the emission of light of the same intensity is continued, are circumstances clearly indicating the employment of heat in some way in the production of light. A body of gas must be raised to a particular temperature to enable it to combine with oxygen, and to evolve light and heat. But one of the most curious circumstances connected with this inquiry is the different proportion which is maintained in different instances between the degree of heat required for combustion, and that produced by the combustion. (See Sir H. Davy's paper, *Phil. Trans.* 1817, Part 1. pp. 48, 52.) These differences would seem very difficult to explain or account for on any known principles; but if I rightly apprehend the author's meaning, it would seem by no means an improbable conclusion, that a portion of the heat disappears as heat, and becomes the latent heat of the light: of this inference, however, I only speak doubtfully.

(12.) I have made these various brief remarks, being fully aware that they give nothing like a complete view of the subject; but I am desirous of laying them before the readers of the *Annals*, in the hope that some persons possessing the requisite chemical knowledge will be induced to give a more complete examination to that very interesting topic of inquiry, the connexion between the colour and heating power of the light, the radiant heat, and the chemical or electro-chemical nature of the process which evolves them, and of the substances from which they are produced.

## ARTICLE II.

*A List and Description of some Species of Shells not taken Notice of by Lamarck.* By John Edward Gray, Esq. MGS.

(Continued from p. 140.)

**EMARGINULA** *Sicula*. Testa subconvexa conica, albida, tenuis, costellis longitudinalibus striisque minutis transversis cancellata; verticè recurvo, subcentrali; apertura ovata; fissura angustissima elongata.

*Emar. octoradiata*. a. tricarinata, *Born.* t. 18, f. 62.  $\beta$ . P. octo-radiata, *Gmelin, Yäst.* 532, f. 11.

*Emar. squamata*. Testa subconvexa conica; costellis longitudinalibus inaequalibus, confertis, squamatis; vertice recurvo subcentrali; margine crenato; fissura brevissima.

*Emar. notata*. Patella notata, *Lin. Chemn.* v. vig. 25, f. C. D.

*Emar. elongata*. Testa subconvexa conica, pellucida albida; striis confertis longitudinalibus, transversisque cancellata; vertice recurvo submarginali; apertura oblonga; fissura brevissima.

**FISSURELLA cancellata**. Patella græca, *Montague*.

*Fis. crenulata*. *Sow. gen.*

*Fis. ventricosa*. Patella ventricosa, *Gmelin*.

*Fis. clypeiformis*. *Sow. gen.*

**PILEOPSIS rosea**. Testa obliquè depresso conica; apertura orbiculato-ovata, intus rosea, long. 1 unc.

*Pil. mitrula*; *subrufa*; *pennata* et *squamiformis* should be removed to the second section, to which also belong

*Pil. crenulata*. Testa rotunda, obliquè conica, rufo concentricè sublamellata, densè radiatim striata; apice incurvo subspirali; margine minutè crenulato.

*Pil. albida*. Testa rotundata, obliquè conica, albida, concentricè substriata, dense radiatim striata; vertice recurvo acuto.

**CALYPTREÆ** *Dillwynii*. Patella equestris, *Dillw.* C. equestris. *Lam.* is P. Neptuni, *Dillw.* C. Tectum Chinensis appears to be a variety of the former.

*Calypt. auricula*. Patella auriculata, *Gmelin*. Patella duplicata, *Mawe Cat.* Calypt. extintorium, *Sow.* not *Lam.*

*Calypt. puncturata*. Testa orbiculata-tenuis, albido-fusca, nigro-punctata, levis, irregulariter subcostata, margine sinuato angulato; vertice recurvo, subcentrali.

*Calypt. spinosa*. *Sow. gen.* f. 4. f. 7.

*Calypt. striata*. Testa ovato-orbiculata, convexo-conica, alba, densè striata; apice recurvo acuto; margine crenulato.

*Calypt. costata*. Testa ovato-orbiculata, crassa, convexo-conica, pallidè fusca, radiatim striata et obliquè irregulariter costata; apice acuto recurvo.

*Calypt. albida.* Patella Chinensis, *Montague*, t. 13, f. 4.  
*β rosea*, intus rosea.

*Calypt. lineata.* Mitella Chineusis alba, *Martini*, t. 13, f. 121, 122. *Pet. Gaz.* t. 21, f. 11.

*Calypt. undulata.* Calyp. extincitorium, *Lam.*? Mitella Chinensis undulata, *Mart.* t. 13, f. 123, 124. *List.* t. 546, f. 39.

*Calypt. alba.* Testa subdepresso-conica, albida, linea spirali fusca notata, concentricè substriata, subtüberculata; apice subspirali anfractu unico; columella perforata.

*Calyptæa comma notata.* *Sow. gen.*

*ANCYLUS Spina Rosæ*, *Drap.* and *Lam.* is a species of crustaceous animal, and should therefore be excluded from the list of shells.

*BULLÆA orientalis.* Testa ovata albida pellucida. Bullæa aperta similis sed ovata.

*Bulla lignaria*, *Bulla lignaria*, *Lam.* on account of the form of the shell, gizzard, and animal, should certainly be placed in this genus, as should also one or two of the fossil species.

*Bullæa alba*, *Hasselt.*

*BULLA australis.* Testa ovato-oblonga, subpellucida, lævis, fusco rufoque marmorata; vertice umbilicato, long. 2 unc. New Holland, *Berry.* South Seas, *Barnard, Capt. G. King.*

*Bulla elegans.* Testa ovato-cylindrica, albido-lutea, pellucida, densè spiraliter striata; vertice umbilicato; columella costato-marginata; apertura patula, long. 3-4 unc. Mare Britannicum et Mediterraneum.

*Bulla Wallisii.* Testa ovata, oblonga, lutea, pellucida, minutissimè spiraliter striata, concentricè substriata; margine columellæ subreflexo albo; vertice imperforato; apertura posticè coarctata; long. 1-4 unc. Nov. Hollandiæ, *Capt. Wallis.*

*Bulla Savigniana.* Testa ovato-oblonga, lutea, tenuis; pellucida, lævis; vertice imperforato; apertura angusta; margine columellæ subreflexa; long. 1-2 unc. Red Sea, *J. E. Savigny.*

These three shells are allied to *B. hydati*s, *Montague*, and there are several other distinct species in the Museum.

*Bulla lineata.* Testa ovato-oblonga, pellucida, densè spiraliter striata, alba; fasciis duabus spiralibus, et lineolis coccineis concentricis ornata; spira conica; apertura elongata, integra. *β* spira depressa, long. 2-3 unc. New Holland, *Mr. E. Barnard.*

*Bulla nitidula*, *Dillwyn.* Priori affinis.

*Bulla soluta*, *Dillw.*

*Bulla solitaria*, *Say.*

*Bulla*, *Say.*

*TESTACELLA scutulum*, *Sow. gen.* f. 3, 6.

*Test. ambigua*, *Fer.* t. 8, f. 4. *Parmacella calyculus*, *Sow. gen.*

*VITRINA Cuvieri.* Helicarion, *Fer.* t. 9, f. 8, t. 9, f. 1, 2.

*V. Freycinetti.* Helicarion, *Fer.* t. 9, f. 3, 4.

*V. brevis.* Helicolimax, *Fer.* t. 9, f. 2.

- V. Lamarckii*. Helicolimax, Fer. t. 9, f. 9.  
 • *V. Pyrenaica*. Helicolimax, Fer. t. 9, f. 3.  
*V. annularis*. Helicolimax, Fer. t. 9, f. 7.  
*V. pellicula*. Helicolimax, Fer. t. 9, A f. 5, 6, 7.  
*HELIX brevipes*, Drap. Fer. t. 10, f. 1.  
*H. rufa*. Fer. t. 10, f. 2.  
*H. Cafra*. Fer. t. 9, A f. 8.  
*H. globulosa*. Fer. t. 25, f. 3, 4.  
*H. versicolor*, Born. Fer. t. 17, f. 1, 2, 3.  
*H. jöllis*. Fer. t. 17, f. 4.  
*H. zonulata*. Fer. t. 15, f. 1, 2. List, t. 1055, f. 4.  
 • *H. conformis*. Fer. t. 25, A f. 10.  
*H. crispata*. Fer. t. 16, f. 7, 8; t. 25, f. 7, 8.  
*H. cincta*, Muller. Fer. t. 22, f. 7, 8.  
*H. ligata*, Muller. Fer. t. 20, f. 1—4; t. 24, f. 4.  
*H. prunum*. Fer. t. 26, f. 7, 8, 9. South Sea.  
*H. gilvus*. Fer. t. 21.  $\beta$  f. 1.  
*H. gyrostoma*. Fer. t. 32, f. 5, 6. Tripoli.  
*H. addita*. Fer. t. 25, B. f. 2, 3.  
*H. torulus*. Fer. t. 27, f. 3, 4. New Holland. Teneriff.  
*H. contusa*. Fer. t. 31, f. 1; t. 39, B. f. 5, 6.  
*H. deformis*. Fer. t. 32, A. f. 1.  
*H. papilla*, Muller. Fer. t. 25, B. f. 5; Chemn. ix. t. 122, f. 104, 105.  
*H. mamilla*. Fer. t. 25, f. 1, 2.  
*H. irregularis*. Fer. t. 28, f. 5, 6.  
*H. maculosa*. Fer. t. 28, f. 9, 10. a. Fer. t. 32, A. f. 9, 10.  
 • *H. Nicæensis*. Fer. t. 28, f. 1, 2.  
*H. ligulata*. Fer. t. 31, f. 2, 3.  
*H. simplex*, Lam. Fer. t. 25, B. f. 6.  
*H. Otaheitana*. Fer. t. 29, f. 4, 5.  
*H. similis*. Fer. n. 262, t. 25, B. f. 1.  
*H. signata*. Fer. t. 30, f. 3. Italy.  
*H. Melitensis*. Fer. t. 25, f. 11, 12. Malta.  
*H. aspersa*, var. *scalaris*, Cornucopia, Born, t. 13, f. 10, 11.  
 Corn. helicina, Shaw. Serpula Cornucopia, Dillw. R. S. 1081.  
*H. guttata*, Oliv. Fer. t. 38, f. 2.  
*H. spiriplana*, Oliv. Fer. t. 38, f. 3, 6.  
*H. marmorata*. Fer. t. 40, f. 8.  
*H. Carseolana*. Fer. t. 41, f. 1.  
*H. circumornata*. Fer. t. 41, f. 2.  
*H. squamosa*. Fer. t. 41, f. 3.  
*H. muralis*. Fer. t. 41, f. 4. Gualt. t. 3, f. F.  
*H. modesta*. Fer. t. 42, f. 1.  
*H. consobrina*. Fer. t. 42, f. 2.  
*H. Pouchet*. Fer. t. 42, f. 3. Adans, t. 1, f. 2.  
*H. cognata*. Fer. t. 44, f. 4.  
*H. aspera*. Fer. t. 44, f. 1—3.  $\beta$  List, t. 94, f. 95.

- H. discolor.* Fer. t. 46, f. 3, 6.  
*H. Lima.* Fer. t. 46, f. 1, 2.  
*H. indistincta.* Fer. t. 38, f. 1.  
*H. formosa.* Fer. t. 47, f. 1. List, t. 74, f. 74?  
*H. sobrina.* Fer. t. 43, f. 6, 7, 8.  
*H. Carmelita.* Fer. t. 32, f. 4.  
*H. orbiculata.* Fer. t. 42, f. 3, 4.  
*H. dentiens.* Fer. t. 49, A. f. 2; t. 48, f. 2.  
*H. punctata,* Boru. t. 14, f. 17, 18. Fer. t. 48, f. 3.  
*H. parilis.* Fer. t. 49, f. 2.  
*H. elevata,* Say. *H. Knoxvilleina.* Fer. t. 49, f. 5, 6.  
*H. Thyroidus,* Say. List, t. 91, f. 91.  $\beta$  edentula.  
*H. avara,* Say.  
*H. auriculata,* Say. List, t. 93, f. 93.  
*H. hirsuta,* Say. List, t. 93, f. 94.  
*H. convexa,* Rafinesque. Fer. t. 50, A. f. 2.  
*H. palliata,* Say. *H. denotata.* Fer. t. 49, A. f. 5.  
*H. clausa,* Raf. *H. reflexa,* Say. Fer. t. 51, f. 2.  
*H. tridentatu,* Say. List, Syn. t. 92, f. 92. Fer. t. 51, f. 51.  
 $\beta$  edentula.  
*H. monodon,* Racket. Lin. Trans. xiii. t. 5, f. 1.  
*H. holosericea.* Fer. t. 51, f. 5.  
*H. plicata,* Say.  
*H. caribanata.* Fer. t. 51, B. f. 3.  
*H. labyrinthica,* Say. Fer. t. 51,  $\beta$ . f. 1.  
*H. Imperator.* Fer. t. 52.  
*H. Soror.* Fer. t. 54, f. 4.  
*H. bidentata.* *H. bidens,* Chemn. ix. t. 126.  
*H. Cobresiana,* Alten. *H. unidentata,* Drap. t. 7, f. 15.  
*H. edentula.* Drap. t. 7, f. 14.  
*H. Pyrenaica.* Drap. t. 13, f. 7.  
*H. Quimperiana.* Fer. t. 75, B. f. 1, 2, 3. a. t. 74, f. 2.  
*H. zonalis.* Fer. t. 70, f. 3.  
*H. exceptiuncula.* Fer. t. 73, A. f. 1; t. 70, f. 1.  
*H. bigonia,* Fer.  
*H. pernobilis,* Martyn. U.C. t. 3, f. 117.  
*H. zodiaca.* Fer. t. 75, f. 2.  
*H. bipartita.* Fer. t. 75, f. A. f. 1.  
*H. dilata.* Fer. Perry Conch. t. 51, f. 4.  
*H. collapsa.* Fer. Perry Conch. t. 51, f. 5.  
*H. divaricata.* Fer. Perry Conch. t. 51, f. 3.  
*H. Senegalensis.* Chemn. ix. t. 109, f. 917, 918.  
*H. concta.* Fer. t. 78, f. 3-4.  
*H. trifasciata.* Chemn. xi. t. 213, f. 3016, 3017.  
*H. unguicula.* Fer. t. 76, f. 3. *H. unguina.* Chemn. ix. t. 125, f. 1098, 1099. a. Fer. f. 4.  $\beta$  lab. int. unidentato. Mus. Cracherode.  
*H. circumdata.* Fer. t. 76, f. 1; t. 77, f. 1.

- H. polygyrata*. Born, t. 14, f. 19, 20. Brazil, Mus. Crach.  
*H. lineata*, Say.  
*H. rudis*. *H. rotundata*, Turton.  
*H. perspectiva*, Say.  
*H. pygmaea*. Drap. t. 8, f. 8, 9, 10.  
*H. umbilicata*. Montague, t. 13, f. 6. *H. rupestris*. Drap.  
 t. 7, f. 7, 8, 9.  
*H. glaphyra*. Say, t. 1, f. 3.  
*H. nitidula*. Drap. t. 8.  
*H. nifidosa*, Fer. *H. nitidula* var. Drap. t. 8, f. 21, 22.  
*H. nitens*. Racket, Lin. Trans. viii.  
*H. subrufescens*. Miller, Ann. Phil. iii. 379.  
*H. arborea*. Say, t. 4, f. 4.  
*H. crystallina*. Drap. t. 8, f. 13—20.  
*H. candida*. Martin, V. Magn. iv. t. 3, f. 22, 23.  
*H. lavipes*, Muller. Fer. t. 92, f. 3, 4, 5, 6.  
*H. leucus*, Lin.  
*H. cicatricosa*, Muller. Chemn. ix. t. 109, f. 923; xi. t. 213,  
 f. 3012, 3013.  
*H. nemorensis*, Muller. Born, t. 16, f. 1, 2.  
*H. Janus bifrons*. Chemn. xi. t. 213, f. 3016, 3017.  
*H. Javacensis*. Fer. t. 92, f. 2.  
*H. exilis*, Muller. Chemn. xi. t. ix. t. 129, f. 1149. Fer. 92, f. 1.  
*H. Rapa*, Muller. Chemn. ix. t. 131, f. 1176.  
*H. Clairvillia*. Fer. t. 91, f. 1. B. f. 2, 3. Manilla, Humph.  
*H. Trochiformis*, Montague. *H. fulva*, Drap.  
*H. aculeata*, Muller. *H. spinulosa*, Montague.  
*H. fasciola*. Drap. t. 6, f. 22, 23, 24.  
*H. limbata*. Drap. t. 6, f. 29.  
*H. Olivieri*, Fer. Drap. t. 7, f. 3, 4, 5.  
*H. Cantiana*. Montague, t. 23, f. 1. *H. palida*, Don.  
*H. strigella*. Drap. t. 7, f. 1, 2, 19.  
*H. villosa*. Drap. t. 7, f. 18.  
*H. glabella*. Drap. t. 7, f. 6.  
*H. rufescens*, Montague, t. 23, f. 2. *H. hispida*, Don.  
*H. sericea*. Drap. t. 7, f. 16, 17.  
*H. scabra*. Chemn. ix. t. 133, f. 1208.  
*H. variegata*. Chemn. ix. t. 133, f. 1207.  
*H. carnicolor*, Fer. Chemn. ix. t. 132, f. 1186, 1187.  
*H. Trochus*, Muller. Chemn. ix. t. 102, f. 1055, 1056.  
*H. subdentata*. Fer. t. 27, f. 1, 2.  
*H. pyramidata*. Drap. t. 5, f. 6.  
*H. conica*. Drap. t. 5, f. 3, 4, 5.  
*H. ochroleuca*. Fer. t. 30, f. 1. Chemn. ix. 126, f. 1105,  
 1106.  
*H. unidentata*. Chemn. xi. t. 208, f. 2049, 2050.  
*H. pellicula*. Fer. t. 105, f. 1.

- H. incerta.* Fer. t. 105, f. 2.  
*H. mirabilis.* Fer. t. 105, f. 3; t. 31, f. 4; t. 104, f. 6, 7.  
*H. Studeriana.* Fer. t. 103, f. 6.  
*H. strobilus.* Fer. t. 103, f. 1.  
*H. avellanea.* Fer. t. 103, f. 4, 5.  
*H. alauda.* Fer. t. 103, f. 2, 3; t. 104, f. 4, 5.  
*H. diaphana.* Fer. t. 104, f. 1.  
*H. Rossiana.* Fer. t. 104, f. 2, 3.  
*H. coniformis.* Fer. t. 108, f. 1.  
*H. subplicata.* Sow. Zool. Jour. i. 56, t. 3, f. 1.  
*H. punctulata.* Sow. Zool. Jour. i. 56, t. 3, f. 2.  
*H. nivosa.* Sow. Zool. Jour. i. 56, t. 3, f. 3.  
*H. nitidiuscula.* Sow. Zool. Jour. i. 57, t. 3, f. 4.  
*H. Portosanctanae.* Sow. Zool. Jour. i. 57, t. 3, f. 5.  
*H. tectiformis.* Sow. Zool. Jour. i. 57, t. 3, f. 6.  
*H. bicarinata.* Sow. Zool. Jour. i. 58, t. 3, f. 7.  
*H. innominata.* Nob. Zool. Jour. i. 58, t. 3, f. 8.  
*CAROCOLLA, Julia.* Helix, Fer. List, t. 83, f. 87?  
*C. angustata.* Helix, Fer. t. 61, f. 1.  
*C. angulata.* Helix, Fer. t. 61, f. 2.  
*C. Lampas.* Helix, Fer. t. 60, f. 2.  
*C. pyrostoma.* Helix, Fer. t. 15, f. 3, 4.  
*C. marginata.* Helix, Fer. t. 63, f. 3—12.  
*C. scabrosa.* Helix, Fer. t. 63, f. 1, 2.  
*C. Pileolus.* Helix, Fer. t. 63, A. f. 1, 2.  
*C. bifasciata.* Trochus, Burrows, t. 27, f. 2.  
*C. Turcica.* Trochus, Chemn. xi. t. 209, f. 2065, 2066.  
*C. cariosa.* Oliv. Voy. t. 31, f. 4. Helix, n. 84, Lam.  
*C. Tripolitana.* Testa orbiculata, suprâ convexo-conica, marginibus carinatis, crenatis, infrâ convexa, imperforata, alba, pellucida, tenuis, concentricè acute corrugata; Peristomate completo albo, reflexo; axis 1-2, diam. 3-4 unc. Tripoli. Ritchie.  
*C. Listeri.* List. t. 66, f. 64. Mus. Brit.  
*C. orientalis,* nob. Testa suprâ convexiuscula, infrâ convexa; umbilicata, cornea, pellucida; anfractibus, 7 v. 8, acutè carinatis, supernè densè concentricè striatis; apertura lineari-lunata, peristomate reflexo, albo; axis 1-4, diam. 1-2 unc. India orientalis?  
*PUPA Auris Leporis.* Auricula Leporis, Lam. n. 4.  
*P. Auris Sileni.* Auricula Sileni, Lam. n. 3.  
*P. Auris cervina.* Helix Auris cervina, Fer. Mawe Brax. f. 4.  
*P. gonistoma.* Helix gonistoma, Fer. Zool. Jour. i.  
*P. Caprella.* Auricula caprella, Lam. Caprella undulata, Guilding. Born, t. 9, f. 3, 4.  
*P. distorta.* Vol. australis, Dillw. Chemn. x. t. 149, f. 1395, 1396.  
*P. Johnii.* Chemn. xi. t. 210, f. 2076, 2077.

*P. Auris vulpina.* Chemn. xi. t. 210, f. 2086, 2087. Struthiolaria crenata, Lam.

*P. melanastoma.* List, t. 29, f. 27. Figura pulcherima; neglecta, Mus. Sloane. Bul. melanastomus, Swain.

*P. Auris Malachi.* Chem. ix. t. 121, f. 1037, 1038.

*P. Auris Bovina.* Chem. ix. t. 121, f. 1039, 1040. Auricula Bovina, Lam.

*P. odontostoma.* Bulimus, Sow. Zool. Journ. i. 59, t. 5, f. 3.

*P. decumana.* List, 588, f. 47. Hel. decumanus, Fer.

*P. Doliolum.* Drap. t. 11, f. 41, 42.

*P. Listeri.* List, t. 31, f. 29. H. Listeri, Fer.

*P. Brasiliensis.* Maue Trav. f. 6. H. Brasiliensis, Fer.

*P. tridens.* Pult. Days. t. 19, f. 2. H. Goodalli, Fer.

*P. cylindra.* Chemn. ix. t. 136, f. 1256, 1257. H. cylindrus, Fer.

*P. truncata.* Cyclost. fasciata, Lam. Ency. Method. t. 461, f. 7.

*P. tortuosa.* Chem. xi. t. 195, A. f. 1882, 1883.

*P. Tristensis.* Balea, nob. Zool. Jour. i. t. 6, f. A.

*P. ventricosa.* Balea, nob. Zool. Jour. i. t. 6, f. B.

*P. Chemnitziana.* Helix, n. 512, Fer. Chemn. ix. t. 112, f. 956.

*P. edentula.* Drap. t. 3, f. 28, 29.

*P. muscorum.* Drap. t. 3, f. 26, 27.

*P. pygmaea.* Drap. t. 3, f. 30, 31.

*P. antivertigo.* Drap. t. 3, f. 32, 33.

*P. vertigo.* Drap. t. 3, f. 34, 35.

*P. contracta,* Say.

*P. exigua,* Say.

*P. ovata.* Vertigo, Say.

*P. pentodon.* Vertigo, Say.

I have removed several of Lamarck's *Auriculæ* to this genus, as they agree better with his character, and with some of the species that he has placed in it himself, than with any of the former genus.

*CLAUSILIA bidens.* Drap. t. 4, f. 5, 7. Turbo laminatus, Montague.

*C. ventricosa.* Drap. t. 4, f. 14.

*C. Montagu.* Turbo biplicatus. Montague, t. 11, f. 5.

*C. solida.* Drap. t. 4, f. 8, 9. T. labiatus, Montague.

*C. plicata.* Drap. t. 4, f. 15, 16.

*C. dubia.* Drap. t. 4, f. 10.

*C. Rolphii,* nob. Med. Rep. H. Everetti. Miller, Ann. Phil. iii. 377?

*BULIMUS metaformis.* Helix, Fer. t. 108, f. 2.

*B. maxima.* Cochlogena maxima, Sow.

*B. ventricosus,* Brug. (not Drap.) Chemn. ix. f. 1007, 1008.

*B. decoratus.* Helix, t. 112, f. 3, 4. List, t. 13, f. 8.

- B. Dufresnii*. Leach, Zool. Misc. ii. t. 154.  
*B. Taunaisii*. Helix, Fer. t. 113, f. 4, 5.  
*B. papyraceus*. Helix, Mawe Intrôd. t. 1, f. 7.  
*B. septenarius*. Helix, n. 46. Fer. Pet. Gaz. t. 17, f. 4.  
*B. iostomus*. Sow. Zool. Jour. i. 58, t. 5, f. 1.  
*Bul. strigatus*, Brug. Helix, Fer.  
*B. striatulus*, Brug. Helix, Fer.  
*B. flammeus*, Brug. Chemn. ix. f. 1024, 1025.  
*B. stramineus*. Bulimulus stramineus, Guilding, Lin. Trans. xiv. List, t. 8, f. 3.  
*B. rufescens*. Testa ovato-conica, perforata, glabra, minutissimè striata, luteo-albida; apice acuto fusco. Peristomate simplici, long. 1 unc. Jamaica.  
*B. Bontia*. Helix Bontia, Chemn. ix. t. 134, f. 1216, 1217.  
*B. Columba*, Brug. Seba, t. 71, f. 6.  
*B. levis*, Brug. Chemn. ix. t. 111, f. 940, 949.  
*B. trifasciatus*, Brug. Bul. zonatus, Sow. Helix trifasciatus, Chemn. ix. t. 134, f. 1215. Helix trizonatus, Fer.  
*B. lineatus*, Brug. Chemn. ix. t. 136, f. 1263.  
*B. Goodalli*. Helix Goodalli, Miller, Ann. Phil. iii. Helix Clavulus, Fer. n. 381?  
*Bulimus pulcher*. Testa ovato-conica, tenuis, albida; fasciis tribus purpureo-fuscis ornata; anfractibus convexiusculis. Peristomate simplici, labio interiori roseo long. 1-2 unc.  
*Bulimus cylindricus*. Testa conico-cylindrica, perforata, albida, densè concentricè striata, fasciis 6 fuscis interruptis ornata; anfractibus 9 v. 10; convexiusculis; apertura suborbiculata; peristomate tenui, long. 6-10; diam. 3-10 unc.  
*Bulimus Kingii*. Testa conico-ovata, perforata, albida, pellucida transverse nigro fusco lineolata; anfractibus convexiusculis; apertura spiræ longitudine; peristomate tenui, intus purpureo nigro, long. 1, diam.  $\frac{1}{2}$  unc. New Holland, Capt. King.  
*ACHATINA exarata*. Bulla exarata. Chemn. ix. t. 120, f. 1031, 1032.  
*A. melanostoma*, Sw. H. regina, Fer. t. 119, f. 3, 4.  $\beta$  sinistra. *A. perversa*, Sw.  
*A. vittata*, Sw.  $\beta$  sinistra.  
*A. fulvescens*. List, t. 582, f. 35 a. Born, t. 10, f. 2.  
*A. marginata*, Sw. Illust. 30.  
*A. rosea*. List, t. 1059, f. 4 (non *Pupa goniosoma*). Helix, Fer. t. 136, f. 89.  
*A. striata*. Chemn. ix. t. 120, f. 1030. Helix, Fer. n. 557.  
*A. Boreti*. Helix, n. 358. Fer. t. 136, f. 1-5.  
*A. decora*. Helix, Fer. Chemn. xi. t. 213, f. 3014, 3015.  $\beta$  dextra.  
*A. lugubris*. Helix, Fer. Chemn. xi. t. 209, f. 2059, 2060.  
*A. Terebraster*. Bulimus Terebraster, Lam. List. t. 20, f. 15.  
*A. octona*. Bulimus octonus, Lam. Chemn. ix. t. 136, f. 1264.

I have removed these two species, because they have the truncated columella of this genus, and are very nearly allied to *A. acicula*, as are also the two following.

*A. sulcata*. Testa turrata, pellucida, cornea, apice obtusa, anfractibus 8 v. 9 convexis, medio concentricè sulcatis, basi lævibus; labro tenui; long. 7-10, diam. 2-10 unc.

*A. nitens*. Testa ovato-conica, turrata, hyalina, cornea, lævi polita, apice obtusiuscula; anfractibus 8 convexis; apertura vata, peristomate tenui, axis 7-10, diam. 3-10 unc.

*Succinea tigrina*, Leseuer. Fer. t. 11, A. f. 4.

*S. ovalis*, Say. Fer. t. 11, A. f. 1.

*S. australis*. Helix, 11. Fer. t. 11, f. 11

*S. campestris*, Say. Fer. t. 11, f. 12.

*S. angularis*. Helix, n. 13. Fer. t. 11, A. f. 5.

*S. sulculosa*. Helix, n. 14. Fer. t. 11, A. f. 6.

*Partula*, Ferussac.

Testa ovata, spira conica. Apertura longitudinalis, anticè integerima, peristomate reflexo; columella anticè callosa.

Animal. Tentacula 2 retractilia, apice oculata.

This genus is most nearly allied to Lamarck's *Auricula*, but the animal has retractile instead of contractile tentacula, and pedicelled instead of sessile eyes.

*P. pudica*, Fer. Chemn. ix. t. 121, f. 1042. List, t. 24, f. 22.

*P. australis*, Fer. Chemn. ix. t. 121, f. 1044.

*P. unidentata*, Sow.

*P. gibba*, Fer.

*P. fragilis*, Fer.

*P. otakeitana*, Fer. Chemn. ix. f. 950, 951.  $\beta$  dextrorsa.

*P. auricula*, Fer.

*Auricula lineata*. Drap. t. 3, f. 20, 21.

*A. corticaria*. Odostonia. Say, t. 4, f. 5.

*A. plicatus*. Scarabus, n. 2, Fer. List, t. 577, f. 32.

*A. Petiverianus*. Scarabus, n. 3, Fer. Pet. Gaz. t. 4, f. 10.

*A. ponderosa*. Fer, n. 4. Mus. Kerc. f. 412.

*A. bidentata*. Fer. n. 9. Vol. bidentata, Montague, t. 30, f. 4.

*A. alba*. Fer. n. 10. Vol. alba, Montague, t. 14, f. 27.

*A. ornata*. Fer. n. 11.

*A. Matoni*. Vol. fluviatilis, Maton, Lin. Trans.

*A. bidentatus*. Melampus, Say.  $\beta$  lineatus.

*A. obliquus*. Melampus, Say.

*A. fabula*. Fer. n. 24.

*A. nucleus*. Fer. n. 26. Helix nucleus, Gmelin.

*A. bullaoides*. Vol. bullaoides, Montague, t. 30, f. 4. Tornatella, n. 7, Fer.

*A. pedipes*. Tornatella pedipes, Lam. Adams, t. 1, f. 4.

*A. mirabiles*. Pedipes, n. 2, Fer.

*A. ovulus*. Pedipes, n. 3, Fer.

*A. affinis*. Pedipes, n. 4, Fer.

## ARTICLE III.

*On the Action of finely divided Platinum, on Gaseous Mixtures, and its Application to their Analysis.* By William Henry, MD.

SEVERAL years have elapsed since the President of the Royal Society, in the further prosecution of those Researches on Flame, which had already led him to the most important practical results, discovered some new and curious phænomena in the combustion of mixed gases, by means of fine wires of platinum introduced into them at a temperature below ignition. A wire of this sort being heated much below the point of visible redness, and immersed in a mixture of coal gas and oxygen gas in due proportions, immediately became white hot, and continued to glow until all that was inflammable in the mixture was consumed. The wire, repeatedly taken out of the mixture and suffered to cool below the point of redness, instantly recovered its temperature on being again plunged into the mixed gases. The same phænomena were produced in mixtures of oxygen with olefiant gas, with carbonic oxide, with cyanogen, and with hydrogen; and in the last case there was an evident production of water. When the wire was very fine, and the gases had been mixed in explosive proportions, the heat of the wire became sufficiently intense to cause them to detonate. In mixtures, which were non-explosive from the redundancy of one or other gas, the combination of their bases went on silently, and the same chemical compounds were formed as by their rapid combustion.†

Facts analogous to these were announced, in the autumn of last year, by Prof. Döbereiner of Jena, with this additional and striking circumstance, that when platinum in a spongy form is introduced into an explosive mixture of oxygen and hydrogen, the metal, even though its temperature had not been previously raised, immediately glows, and causes the union of the two gases to take place, sometimes silently, at others with detonation. It is remarkable, however, that platinum in this form, though so active on mixtures of oxygen and hydrogen, produces no effect, at common temperatures, on mixtures of oxygen with those compound gases, which were found by Sir Humphry Davy to be so readily acted upon by the heated wire.‡ Carbonic oxide appears, indeed, from the statement of MM. Dulong and Thenard,§ to be capable of uniting with oxygen at the tempera-

\* From the Philosophical Transactions, for 1824, Part II.

† Philosophical Transactions, 1817, p. 77.

‡ Döbereiner in Ann. de Chim. et de Phys. xxiv—xcvi.

§ Ditto xxiii, 442.

ture of the atmosphere, by means of the sponge; but though this is in strictness true, yet the combination, in all the experiments I have made, has been extremely slow, and the due diminution of volume has not been completed till several days have elapsed. On mixtures of olefiant gas, of carburetted hydrogen, or of cyanogen, with oxygen, the sponge does not, by any duration of contact, exert the smallest action at common temperatures.

It was this inefficiency of the platinum sponge on the compounds of charcoal and hydrogen in mixture with oxygen, while it acts so remarkably on common hydrogen, and also, though slowly, on carbonic oxide, that suggested to me the possibility of solving, by its means, some interesting problems in gaseous analysis. I hoped, more especially, to be able to separate from each other the gases constituting certain mixtures, to the composition of which approximations only had been hitherto made, by comparing the phenomena and results of their combustion with those which ought to ensue, supposing such mixtures to consist of certain hypothetical proportions of known gases. It might, for instance, be expected, that from a mixture of hydrogen and carburetted hydrogen with oxygen, the platinum sponge would cause the removal of the hydrogen, leaving the carburetted hydrogen unaltered. To ascertain this, and a variety of similar facts, I made artificial mixtures of the combustible gases in known volumes; and submitted them, mixed with oxygen, sometimes to contact with the sponge, and sometimes with the balls made of clay and platinum, described by Professor Döbereiner.\*

#### SECT. I.—*On the Action of finely divided Platinum on Gaseous Mixtures at common Temperatures.*

##### 1. *Mixtures of Hydrogen and Olefiant Gases with Oxygen.*

When to equal volumes of olefiant gas, and an explosive mixture (which is to be understood, whenever it is so named, as consisting of two volumes of hydrogen and one of oxygen gases), one of the platinum balls, recently heated by the blow-pipe, and allowed to cool during eight or ten seconds, is introduced through mercury, a rapid diminution of volume takes place; the whole of the hydrogen and oxygen gases is condensed; but the olefiant gas is either not at all, or very little

\* The proportions which I used, but which perhaps are not of much importance, were two parts of fine china clay, and three parts of spongy platinum mixed with water into a paste, which was moulded into small spherules, about the size of peas. The sponge, best adapted to the purpose of acting on mixed gases, is obtained by using a little pressure to the ammonia-muriate, after putting it into the crucible. If too light and porous, the sponge is apt to absorb mercury by being repeatedly passed through it, and to become amalgamated. In order that the balls or sponge might be removed after their full action, they were fastened to pieces of platinum wire.

acted upon. In a few experiments, when the tube was narrow, and the quantity of mixed gases small, the olefiant gas escaped combustion entirely; but, in general, an eighth or tenth of it was converted into water and carbonic acid. It is difficult, however, to state the precise proportion of any gas which, when added to an explosive mixture, renders the latter insensible to the action of the balls or sponge; for much depends on their temperature when introduced into the gaseous mixture, the diameter of the containing vessel, and other circumstances, which, in comparing different gases, should be so regulated as to be equal in every case.

When the proportions of the gases are changed, so that the explosive mixture exceeds in volume the olefiant gas, there is a more decided action upon the latter, manifested by an increased production of carbonic acid. Thus, for example, the explosive mixture being to the olefiant as  $2\frac{1}{2}$  to 1, about one-fourth of the olefiant gas was consumed; and by increasing the proportion of the explosive mixture, the olefiant gas was still more acted upon. On using oxygen sufficient to saturate both the hydrogen and the olefiant gases, the ball acted much more rapidly; in several instances it became red hot; all the hydrogen was consumed; and the whole of the olefiant gas was changed into water and carbonic acid. In this case the use of the sponge is inadmissible, as it kindles the gases, and occasions their detonation.

## 2. *Mixtures of Hydrogen and Carburetted Hydrogen Gases with Oxygen.*

When carburetted hydrogen, procured from stagnant water, was added to an explosive mixture, in various proportions between equal volumes, and ten of the former to one of the latter, the action of the hydrogen and oxygen on each other took place as usual, on admitting one of the balls. When, reversing the proportion, the explosive mixture was made to exceed the carburetted hydrogen, but not more than four or five times, the latter gas was entirely unchanged. With a larger proportion of the explosive mixture carbonic acid was always found to have been produced; but still the carburetted hydrogen was very imperfectly consumed, and fully three-fourths of it were generally found to have escaped unburned.

When, to a mixture of hydrogen and carburetted hydrogen, oxygen enough was added to saturate both gases, the effect of the sponge was found to vary with the proportion of the simple hydrogen. In several cases, where the hydrogen did not exceed the carburetted hydrogen more than four times, the latter gas remained unchanged; when in larger proportion, there was a decided action upon the carburetted hydrogen. But it was much more easy to regulate the action of the balls upon such a mixture, so as to act upon the hydrogen and oxygen only, than

in the case of olefiant gas, which, under similar circumstances, is always more largely converted into water and carbonic acid.

### 3. *Mixtures of Hydrogen and Carbonic Oxide with Oxygen.*

The addition of one volume of carbonic oxide to two volumes of an explosive mixture produces a distinct effect in suspending the action of the platinum balls, and even of the spongy metal itself. The action of the gases upon each other still, however, goes on slowly, even when the carbonic oxide exceeds the explosive mixture in volume; and after the lapse of a few days, the oxygen is found to have disappeared, and to have partly formed water, and partly carbonic acid. I made numerous experiments to ascertain whether the oxygen, under these circumstances of slow combustion, is divided between the carbonic oxide and the hydrogen, in proportions corresponding to the volumes of those two gases. The combustible gases being in equal volumes, and the oxygen sufficient to saturate only one of them, it was found that the oxygen, which had united with the carbonic oxide, was to that which had combined with the hydrogen, as about 5 to 1 in volume. Increasing the carbonic oxide, a still larger proportion of oxygen was expended in forming carbonic acid. On the contrary, when the hydrogen was increased, a greater proportional quantity of oxygen went to the formation of water. But it was remarkable, that when the hydrogen was made to exceed the carbonic oxide four or five times, less oxygen in the whole was consumed than before; the activity of the carbonic oxide appearing to have been diminished, without a corresponding increase in that of the hydrogen.

In cases, where the proportion of the carbonic oxide to the explosive mixture was intentionally so limited, that the platinum ball was capable of immediately acting upon the latter, the carbonic oxide was always in part changed into carbonic acid, the more abundantly as its volume was exceeded by that of the explosive mixture. Increasing the oxygen, so that it was adequate to saturate both gases, and causing the hydrogen to exceed the carbonic oxide in volume, a speedy action was always exerted by the ball, and the whole of the combustible gases was silently converted into water and carbonic acid. The introduction of the platinum sponge into such a mixture was almost always found to produce detonation.

### 4. *Mixtures of Hydrogen and Cyanogen with Oxygen.*

When one of the platinum balls, after being recently heated, is introduced into cyanogen and explosive mixture in equal volumes, no apparent action takes place. With half a volume of cyanogen there is a slight diminution; and as we reduce the proportion of that gas, the action of the elements of the explosive mixture on each other becomes more and more distinct.

There is not, however, as with carbonic oxide, any production of carbonic acid; but in the course of a few minutes the inside of the tube becomes coated with a brownish substance, soluble in water, and communicating to it the same colour; having a smell resembling that of a burnt animal substance; and yielding ammonia on the addition of a drop or two of liquid potash. It was produced in too small a quantity to enable me to submit it to a more minute examination; but its characters appeared to resemble those of a product, obtained by M. Gay-Lussac, by mixing cyanogen with ammoniacal gas.\*

If oxygen be added to a mixture of hydrogen and cyanogen, in quantity sufficient to saturate both the gases, it is still necessary, in order that an immediate effect should be produced by the sponge, that the hydrogen should exceed the cyanogen in volume. A decided action then takes place; an immediate absorption ensues; fumes of nitrous acid vapour appear, which act on the surface of the mercury; and, after transferring the gas into a dry tube, carbonic acid is found to have been produced, equivalent in volume to double that of the cyanogen.

#### 5. *Effect of adding various other Gases to an Explosive Mixture of Hydrogen and Oxygen.*

It had been already ascertained by Prof. Döbereiner, that one volume of oxygen, diluted with 99 volumes of nitrogen, is still sensible, when mixed with a due proportion of hydrogen, to the action of the sponge.† Carbonic acid, also, even I find when it exceeds the explosive mixture ten times, retards only in a slight degree the energy of the sponge. Oxygen, hydrogen, and nitrous oxide gases, when employed to dilute an explosive mixture, are equally inefficient in preventing the mutual action of its ingredients. Ammonia may be added in ten times the volume of the explosive mixture, and muriatic acid gas in six times its volume, with no other effect than that of rendering the action of the sponge less speedy.

#### 6. *Mixtures of Carbonic Oxide and Carburetted Hydrogen with Oxygen.*

When mixtures of these gases are exposed to the sponge, the carburetted hydrogen seems to stand entirely neutral. The carbonic oxide is converted into carbonic acid, in the same gradual manner as if it had been mixed with oxygen only, and the carburetted hydrogen remains unaltered.

\* *Annales de Chimie*, xcv. 196.

† In analysing atmospheric air by adding hydrogen to it, and acting on the mixture by a platinum ball, I have generally obtained a diminution indicating more than 21 per cent. of oxygen. This I find to be owing to the absorption of a small quantity of nitrogen by the ball, especially when, after being heated, it has been rapidly passed hot through the mercury.

7. *Mixtures of Hydrogen, Carburetted Hydrogen, and Carbonic Oxide with Oxygen.*

In mixtures of these gases, it is of little consequence whether the oxygen be sufficient for the hydrogen and carbonic oxide only, or be adequate to the saturation of all three. The circumstance, which has the greatest influence on the results of exposing such mixtures to the sponge, is the proportion which the simple hydrogen bears to the other gases, and especially to the carbonic oxide; for in order that there may be any immediate action, the hydrogen should exceed the other gas in volume. In that case the hydrogen is converted into water, and the carbonic oxide into carbonic acid; but the carburetted hydrogen, unless the excess of hydrogen be very considerable, remains unaltered. If the proportion of hydrogen be so small, that no immediate action is excited by the sponge, the ingredients of the mixture nevertheless act slowly upon each other; and after a few days, the whole of the hydrogen and carbonic oxide are found to have united with oxygen, and the carburetted hydrogen to remain of its original volume.

8. *Mixtures of Hydrogen, Carbonic Oxide, and Olefiant Gases with Oxygen.*

When the oxygen, in a mixture of these gases, is sufficient to saturate the first two only, and the proportion of hydrogen is so adjusted that the action of the sponge is not very energetic, the hydrogen and carbonic oxide only are acted upon; but if the diminution of volume, which the sponge produces, be rapid and considerable, part of the olefiant gas is converted into water and carbonic acid. This effect on olefiant gas takes place still more readily, if the oxygen present be adequate to the saturation of all three combustible gases.

It is remarkable, that if to a mixture of hydrogen, carbonic oxide, and oxygen, in such proportions that the sponge would act rapidly in producing combination, olefiant gas be added, the action of the gases on each other is suspended. Thus 20 measures of carbonic oxide, 31 of hydrogen, and 28 of oxygen, were instantly acted upon by the sponge; but the addition of 20 measures of olefiant gas to a similar mixture entirely suspended its efficiency. By standing fourteen days, rather more than half the carbonic oxide was acidified, and about one-twelfth of the hydrogen was changed into water, but the olefiant gas remained unaltered.

9. *Mixtures of Hydrogen, Carbonic Oxide, Carburetted Hydrogen, and Olefiant Gases with Oxygen.*

In mixtures of these four gases with oxygen, it was found, by varying the proportion of hydrogen, that hydrogen and

carbonic oxide are most easily acted upon; then olefiant gas; and carburetted hydrogen with the greatest difficulty. When the action of the sponge was moderately intense, only the hydrogen and carbonic oxide were consumed, or at most the olefiant gas was but partially acted upon. Adding more hydrogen, so as to occasion a more rapid diminution, the olefiant gas also was burned; but the carburetted hydrogen always escaped combustion, unless the hydrogen were in such proportion that the ball or sponge became red hot.

From the facts which have been stated, it appears that when the compound combustible gases mixed with each other, with hydrogen, and with oxygen, are exposed to the platinum balls or sponge, the several gases are not acted upon with equal facility; but that carbonic oxide is most disposed to unite with oxygen; then olefiant gas; and lastly, carburetted hydrogen. By due regulation of the proportion of hydrogen, it is possible to change the whole of the carbonic oxide into carbonic acid, without acting on the olefiant gas or carburetted hydrogen. With respect indeed to olefiant gas, this exclusion is attended with some difficulty, and it is generally more or less converted into carbonic acid and water. But it is easy, when olefiant gas is absent, so to regulate the proportion of hydrogen, that the carbonic oxide may be entirely acidified, and the whole of the carburetted hydrogen be left unaltered. This will generally be found to have been accomplished, when the platinum ball has occasioned a diminution of the mixture, at about the same rate as atmospheric air is diminished by nitrous gas, when the former is admitted to the latter in a narrow tube.

SECT. II.—*On the Effect of finely divided Platinum on Gaseous Mixtures at increased Temperatures.*

The effect of varying the proportion of free hydrogen to the compound combustible gases, on the degree of action which is excited by the platinum sponge, will perhaps admit of being explained, by examining the facts that have been stated, in connexion with the degrees of combustibility of the compound gases under ordinary circumstances. The precise degree of temperature at which any one of them burns is not known, on account of the imperfection of our present methods of measuring high degrees of heat. It has been ascertained, however, by Sir Humphry Davy,\* that at a heat between that of boiling mercury, and that which renders glass luminous in the dark, hydrogen and oxygen gases unite silently, and without any light being evolved; that carbonic oxide is as inflammable as hydrogen; that olefiant gas is fired by iron and charcoal heated to redness;

\* On Flame, 8vo. p. 72.

but that carburetted hydrogen, to be inflamed, requires that the wire should be white hot. Now this is precisely the order in which the three compound gases require hydrogen to be added to them, in order to be rendered susceptible of being acted upon by the platinum sponge; carbonic oxide being acted upon with the smallest proportion of hydrogen; olefiant gas requiring more hydrogen; and carburetted hydrogen a still larger proportion.

It is extremely probable, then, that the temperature, produced by the union of the hydrogen and oxygen forming part of any mixture, is the circumstance which determines the combustible gases to unite, or not, with oxygen by means of the sponge. It was desirable, however, to ascertain the exact temperature at which each of those three gases unites with oxygen with the intervention of the spongy platinum. For this purpose the gases, mixed with oxygen enough to saturate them, were severally exposed in small retorts containing a platinum sponge, and immersed in a mercurial bath, to a temperature which was gradually raised till the gases began to act on each other. In this way the following facts were determined.

1st. Carbonic oxide began to be converted into carbonic acid at a temperature between  $300^{\circ}$  and  $310^{\circ}$  Fahrenheit. By raising the temperature to  $340^{\circ}$ , and keeping it at that point for 10 or 15 minutes, the whole of the gas was acidified, the condensation of volume in the mixture being equivalent to the oxygen which had disappeared.

2dly. Olefiant gas, mixed with sufficient oxygen, and in contact with the sponge, showed a commencement of decomposition at  $480^{\circ}$  Fahrenheit, and was slowly but entirely changed into carbonic acid by a temperature not exceeding  $520^{\circ}$  Fahrenheit. MM. Dulong and Thenard\* state the same change to take place at  $300^{\circ}$  cent. =  $572^{\circ}$  Fahrenheit; but having repeated the experiment several times, I find no reason to deviate from the temperature which I have assigned.

3dly. Carburetted hydrogen, exposed under the same circumstances, was not in the least acted upon by a temperature of  $555^{\circ}$  Fahrenheit, the highest to which, by an Argand's lamp, I was able to raise the mercurial bath. This, however, must have been near the temperature required for combination; for on removing the retort from the mercurial bath, and applying a spirit lamp, at such a distance as not to make the retort red hot, a diminution of volume commenced, and continued till all the carburetted hydrogen was silently converted into water and carbonic acid.

4thly. Cyanogen, similarly treated, was not changed at a temperature of  $555^{\circ}$  Fahr. and on applying the flame of a spirit lamp to the tube, it produced no action till the tube began to soften.

\* Ann. de Chim. et de Phys. xxiii. 442.

5thly. Muriatic acid gas, mixed with half its volume of oxygen, began to be acted upon at  $250^{\circ}$  Fahr. Water was evidently formed; and the disengaged chlorine, acting upon the mercurial vapour in the tube, formed calomel, which was condensed, and coated its inner surface.

6thly. Ammoniacal gas, mixed with an equal volume of oxygen, showed a commencement of decomposition at  $380^{\circ}$  Fahrenheit. Water was also in this case distinctly generated; and at the close of the experiment, nothing remained in the tube but nitrogen and the redundant oxygen.

I proceeded, in the next place, to examine the agency of finely divided platinum at high temperatures on those mixtures of gases, which are either not decomposed, or are slowly decomposed, at the temperature of the atmosphere.

When carbonic oxide and hydrogen gases, in equal volumes, mixed with oxygen sufficient to saturate only one of them, were placed in contact with the sponge, and gradually heated in a mercurial bath, the mixture ceased to expand between  $300^{\circ}$  and  $310^{\circ}$  Fahrenheit, and soon began to diminish in volume. On raising the temperature to  $340^{\circ}$ , and keeping it some time at that point, no further diminution was at length perceptible. From the quantity of carbonic acid, remaining at the close of the experiment, it appeared that four-fifths of the oxygen had united with the carbonic oxide, and only one-fifth with the hydrogen. When four volumes of hydrogen, two of carbonic oxide, and one of oxygen, were similarly treated, the hydrogen, notwithstanding its greater proportional volume, was still found to have taken only one-fifth of the oxygen, while four-fifths had combined with the carbonic oxide. These facts show that at temperatures between  $300^{\circ}$  and  $340^{\circ}$  Fahrenheit, the affinity of carbonic oxide for oxygen is decidedly superior to that of hydrogen; as, from the experiments before described, appears to be the case also at common temperatures.

But a similar distribution of oxygen between carbonic oxide and hydrogen does not take place when those three gases are fired together by the electric spark. This will appear from the following table, in which the first three columns show the quantities of gases that were fired, and the last two, the quantities of oxygen that were found to have united with the carbonic oxide and with the hydrogen.

	Before firing.			After firing.	
	Measure of carb. oxide.	Measure of hydrogen.	Measure of oxygen.	Oxygen to carb. oxide.	Oxygen to hydrogen.
Exp. 1	40	40	20	6	14
2	40	20	20	12	8
3	20	40	20	5	15

When equal volumes of carbonic oxide and hydrogen gases,

mixed with oxygen sufficient to saturate only one of them, were exposed in a glass tube to the flame of a spirit lamp, without the presence of the sponge, till the tube began to soften, the combination of the gases was effected without explosion, and was merely indicated by a diminution of volume, and an oscillatory motion of the mercury in the tube. At the close of the experiment, out of twenty volumes of oxygen, eight were found to have united with the carbonic oxide, and twelve with the hydrogen, proportions which do not materially differ from the results of the first experiment in the foregoing table. At high temperature, then, the attraction of hydrogen for oxygen appears to exceed that of carbonic oxide for oxygen; at lower temperatures, especially when the gases are in contact with the platinum sponge, the reverse takes place, and the affinity of carbonic oxide for oxygen prevails.

Extending the comparison to the attraction of olefiant and hydrogen gases for oxygen at a red heat, I found that when six volumes of olefiant, six of hydrogen, and three of oxygen were heated by a spirit lamp till the tube softened, a silent combination took place as before; all the oxygen was consumed; but only half a volume had been expended in forming carbonic acid, which indicates the decomposition of only one quarter of a volume of olefiant gas. On attempting a similar comparison between carbonic oxide and olefiant gas, by heating them with oxygen in the same proportions, the mixture exploded as soon as the glass became red hot, and burst the tube.

The property inherent in certain gases, of retarding the action of the platinum sponge, when they are added to an explosive mixture of oxygen and hydrogen, is most remarkable in those which possess the strongest attraction for oxygen; and it is probably to the degree of this attraction, rather than to any agency arising out of their relations to caloric, that we are to ascribe the various powers which the gases manifest in this respect. This will appear from the following table, the first column of which shows the number of volumes of each gas required to render one volume of an explosive mixture of hydrogen and oxygen uninflammable by the discharge of a Leyden jar; while the second column shows the number of volumes of each gas necessary, in some cases, to render one volume of an explosive mixture insensible to the action of the sponge, and in other cases indicates the number which may be added without preventing immediate combination. In the first column, the numbers marked with an asterisk were determined by Sir Humphry Davy; the remaining numbers in that column, and the whole of the second, are derived from my own experiments.

1 vol. of explosive mixture was rendered incapable of being inflamed by electricity when mixed with

Effect of adding the same gases to 1 vol. of explosive mixture on the action of the sponge.

* About 8 vol. of hydrogen	not prevented by many vols.
6 nitrogen. . . .	ditto.
* 9 oxygen. . . .	not prevented by 10 vol.
* 11 nitrous oxide	ditto.
1: cyanogen. . .	prevented by 1 vol.
1 carb. hydrog.	not prevented by 10 vol.
4 carbonic ox.	prevented by $\frac{1}{2}$ a vol.
* 0: olefiant gas. .	prevented by 1.5 vol.
* 2 muriatic acid	not prevented by 6 vol.
2 ammonia . . .	not prevented by 10 vol.
3 carbonic acid	ditto.

From the foregoing table it appears, that carbonic oxide produces the greatest effect, in the smallest proportion to an explosive mixture of oxygen and hydrogen, in preventing the action of those gases on each other, when exposed to the sponge at temperatures below the boiling point of mercury. In general, those gases which either do not unite with oxygen, or unite with it only at high temperatures, have little effect in restraining the efficiency of the sponge. There is an apparent exception, however, in cyanogen, which it would require more research than I have yet had time to devote to an object merely collateral, to reconcile (if it be capable of being reconciled), with the general principle.

From the fact that carbonic oxide, olefiant gas, and carburetted hydrogen, when brought to unite with oxygen by means of the platinum sponge assisted by heat, undergo this change at different temperatures, it seemed an obvious conclusion, that by exposing a mixture of those gases with each other and with oxygen to a regulated temperature, the correct analysis of such mixtures might probably be accomplished. Mixtures of two or more of the combustible gases were therefore exposed, in contact with oxygen gas and the platinum sponge, in tubes bent into the shape of retorts, which were immersed in a mercurial bath. This bath was gradually heated to the required temperatures, and by proper management of the source of heat, was prevented from rising above that degree.

1st. By subjecting 25 measures of carbonic oxide, 15 of olefiant gas, and 57 of oxygen, in contact with the sponge, to a heat which was not allowed to exceed 350° Fahrenheit till the diminution of volume ceased, all the carbonic oxide was converted into carbonic acid, and the olefiant gas remained in its original volume.

2d. By exposing in a similar manner 20 measures of carbonic oxide, 21 of carburetted hydrogen, and 36 of oxygen, to a tem-

perature below  $400^{\circ}$  Fahrenheit, the carbonic oxide was entirely acidified; and on washing out the carbonic acid by liquid potash, the carburetted hydrogen was found unaltered, mixed with the redundant oxygen.

3d. A mixture of 10 measures of olefiant gas, 10 of carburetted hydrogen, and 58 of oxygen, being heated in contact with the sponge to  $510^{\circ}$  Fahrenheit, the olefiant gas was silently but entirely changed into carbonic acid, while the carburetted hydrogen was not at all acted upon.

4th. By acting with the sponge upon 42 measures of carburetted hydrogen, 22 of carbonic oxide, 22 of hydrogen, and 28 of oxygen, first at a temperature of  $340^{\circ}$  Fahrenheit, which was raised gradually to  $480^{\circ}$ , all the carbonic oxide was changed into carbonic acid, and all the hydrogen into water; but the carburetted hydrogen remained undiminished in quantity, and was found, after removing the carbonic acid, mixed only with the redundant oxygen. In this experiment, the diminution of volume had continued some time before there was any perceptible formation of water, the attraction of carbonic oxide for oxygen appearing to prevail over that of hydrogen. The same precedency in the formation of carbonic acid is always apparent, when carbonic oxide and hydrogen, mixed even with oxygen enough to saturate both gases, are raised to  $350^{\circ}$  Fahrenheit.

By thus carefully regulating the temperature of the mercurial bath, the action of oxygen upon several gases (carbonic oxide, olefiant, and carburetted hydrogen for example) may be made to take place in succession; and by removing the carbonic acid, formed at each operation, it may be ascertained how much of each of the two first gases has been decomposed. The carburetted hydrogen indeed always remains unchanged, and its quantity must be determined by firing it with oxygen by the electric spark. If hydrogen also be present, it is difficult to prevent the olefiant gas from being partially acted upon; but this is of little consequence, as I have shown that it is easy to remove that gas in the first instance by chlorine.\* It may be remarked, that this method of operating on the aeriform compounds of charcoal gives more accurate results than rapid combustion by the electric spark, being never attended with that precipitation of charcoal, which is often observed when the gases are exploded with oxygen. A regulated temperature, also, effects the analysis of such mixtures much more correctly than the action of the sponge or balls, because in the latter case the heat produced is uncertain; and though sometimes adequate to the effect, yet there is always a risk that it may exceed, or fall short of that degree, which is required for the successful result of the analytic process.

\* Philosophical Transactions, 1821, p. 147.

From the facts which have been stated, I derived a method of obtaining carburetted hydrogen gas perfectly free from olefiant gas, hydrogen, and carbonic oxide, and mixed only with a little oxygen, which, had it been necessary to my purpose, might also have been separated. The early product of the distillation of pit-coal was washed with a watery solution of chlorine, and afterwards with liquid potash, to remove a little chlorine that arose into the gas from the solution. The residuary gas was next heated with one-fourth its volume of oxygen, at the temperature of  $350^{\circ}$  Fahrenheit, in contact with the sponge; which converted the carbonic oxide into carbonic acid, and the hydrogen into water. The carbonic acid being removed by liquid potash, there remained only the carburetted hydrogen, the redundant oxygen, and a very minute quantity of nitrogen introduced by the latter gas. Hitherto, I have prepared this gas only in a small quantity, but it would be easy to extend the scale of the operation, and to remove the excess of oxygen by obvious methods.

SECT. III.—*Application of the Facts to the Analysis of Mixtures of the Combustible Gases in unknown Proportions.*

At an early period of the investigation described in the first section, I proceeded to apply the facts of which I was then possessed, to the analysis of a mixture of gases in unknown proportions. For this purpose, I caused a quantity of gas to be collected from coal, by continuing the application of heat to the retorts two hours beyond the usual period, and receiving the gas into a separate vessel. Gas of this quality was purposely chosen, because, from former experience, I expected it to contain free hydrogen, carbonic oxide, and carburetted hydrogen, but no olefiant gas, the production of which is confined to the early stages of the process. After washing it, therefore, with liquid potash, to remove a little carbonic acid, and ascertaining its specific gravity when thus washed to be 308, I proceeded at once to subject it to the new method of analysis.

Having ascertained, by a previous experiment with Volta's eudiometer, that 10 volumes of the gas required for saturation 9 volumes of oxygen, I mixed 43 measures with 43 of oxygen ( $= 41$  pure) and passed a platinum ball, which had been recently heated, into the mixture. An immediate diminution of volume took place, attended with a production of heat, and formation of moisture. The residuary gas, cooled to the temperature of the atmosphere, measured 43.5 volumes. Of these 4.5 were absorbed by liquid potash, indicating 4.5 carbonic acid, equivalent to 4.5 carbonic oxide; the rest, being fired in a Volta's eudiometer with an additional quantity of oxygen, gave 11 volumes of carbonic acid; the diminution being 22 volumes, and the oxygen consumed 22 also, circumstances which prove that 11 volumes

of carburetted hydrogen were consumed by this rapid combustion. But of the loss of volume first observed (viz.  $86 - 43.5 = 42.5$ )  $2.25$  are due to the carbonic acid formed; and deducting this from  $42.5$ , we have  $40.25$ , which are due to the oxygen and hydrogen converted into water; and  $40.25 \times \frac{3}{2} = 26.8$  shows the hydrogen in the original gas. But the sum of these numbers ( $26.8 + 4.5 + 11$ ) being less by  $0.7$  than the volume of gas submitted to analysis, we may safely consider that fraction of a measure to have been nitrogen. The composition then of the mixture will stand in volumes as follows:

Hydrogen .....	26.8	.....	62.32
Carbonic oxide.....	4.5	.....	10.50
Carburetted hydrogen..	11.0	.....	25.56
Nitrogen .....	0.7	.....	1.62
	<hr/> 43.0		<hr/> 100.00

On calculating what should be the specific gravity of a mixture of gases in the above proportions, it was found to be  $.303$ ,\* which coincides, as nearly as can be expected, with the actual specific gravity of the gas submitted to analysis, viz.  $.308$ . To place the correctness of the results beyond question, I mingled the gases in the above proportions, and acted on the artificial mixture in the same manner as on the original gas, when I had the satisfaction to find that the analytical process again gave the true volumes with the most perfect correctness for the hydrogen and carbonic oxide, and within the fraction of a measure for the carburetted hydrogen. Notwithstanding this successful result, which was twice obtained, I should still prefer, for the reason which has been stated, having recourse to a temperature carefully regulated, for the analysis of similar mixtures, in all cases where the hydrogen is in moderate proportion, and where great accuracy is desirable. Whenever (it may again be remarked) olefiant gas is present in a mixture, it should always be removed by chlorine, before proceeding to expose the mixture to the agency of the spongy metal.

It can scarcely be necessary to enter into further details respecting methods of analysis, the application of which to particular cases must be sufficiently obvious, from the experiments which have been described on artificial mixtures. The apparatus required is extremely simple, consisting, when the balls are employed, of graduated tubes of a diameter between  $0.3$  and  $0.6$  of an inch; or, when an increased temperature is used, of tubes bent into the shape of retorts, of a diameter varying with the quantity of gas to be submitted to experiment, which may be

\* In this estimate, the specific gravity of hydrogen is taken at  $.0694$ ; that of carbonic oxide at  $.6722$ ; of carburetted hydrogen at  $.5553$ ; and of nitrogen at  $.9728$ .

from half a cubic inch to a cubic inch or more. These, when in use, may be immersed in a small iron cistern containing mercury, and provided with a cover in which are two holes, one for the tube, and the other for the stem of a thermometer, the degrees of which are best engraved on the glass. The gas is, of course, confined in the tube by keeping the open end immersed in a small basin of mercury.

By means of these improved modes of analysis, I have already obtained some interesting illustrations of the nature of the gases from coal and from oil. I reserve, however, the communication of them, till I have had an opportunity of pursuing the inquiry to a greater extent, and especially of satisfying myself respecting the exact nature of the compound of charcoal and hydrogen, discovered some years ago by Mr. Dalton, in oil gas and coal gas, which agrees with olefiant gas in being condensable by chlorine, but differs from it in affording more carbonic acid and consuming more oxygen.

## ARTICLE IV.

### *Astronomical Observations, 1825.*

By Col. Beaufoy, FRS.

#### *Bushey Heath, near Stanmore.*

Latitude  $51^{\circ} 37' 44.3''$  North. Longitude West in time  $1^{\text{h}} 20.93''$ .

April 19.	Emersion of Jupiter's first satellite . . . . .	{	9 <sup>h</sup> 34' 03"	Mean Time at Bushey.
April 25.	Ingress of Jupiter's third satellite . . . . .	{	9 35 24	Mean Time at Greenwich.
		{	9 08 55	Mean Time at Bushey.
		{	9 10 16	Mean Time at Greenwich.
April 30.	Emersion of Jupiter's second satellite . . . . .	{	9 32 05	Mean Time at Bushey.
		{	9 33 26	Mean Time at Greenwich.
May 6.	Emersion of Jupiter's third satellite . . . . .	{	11 53 20	Mean Time at Bushey.
		{	11 54 41	Mean Time at Greenwich.
April 24.	Immersion of a small star by the moon . . . . .	{	11 32 10.8	Siderial Time.

Observed Transits of the Moon and Moon-culminating Stars over the Middle Wire of the Transit Instrument in Siderial Time.

1825.	Stars.	Transit.
April 29.—	167 Virginis . . . . .	11 <sup>h</sup> 42' 09.28"
	29.—Moon's First or West Limb . . . .	11 55 07.57
	29.—14 Virginis . . . . .	12 10 24.17

ARTICLE V.

*On the Chemical Composition of Sponges.* By John Edward Gray, Esq. MGS.

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

*British Museum.*

IN my paper on Sponges in the Zoological Journal, I observed that the sponges "all appear to be essentially formed after the same manner, that is to say, of longitudinally placed transparent fusiform *spiculae*," and further, that "the fibres are composed of *spiculae* united by a cartilaginous substance." I collected a quantity of *spiculae*, by washing them from a sponge in which they were very large and distinct. I accidentally found that they scratched glass, when rubbed hard against it. My attention having been attracted to this fact, having before considered them as mostly composed of carbonate or phosphate of lime, I applied to my friend Mr. Children, stating the circumstance, when he informed me that he had just observed that a sponge-like body lately given to him by Mr. Heuland, which proved to be a *Tethya* (a genus, which, in the before referred to paper, I stated to be formed almost entirely of *spiculae*), consisted wholly of pure *silica*, and a little animal matter. On subjecting some sponges to experiment, considerable quantities of *silica* were found in the ashes of *Spongilla fluvialilis*, *Spongia tomentosa*, and two or three other allied species; and a small quantity in *Spongia officinalis*, and a distinct trace, sufficient to form a globe before the blowpipe, in the ashes of a piece of the axis of *Gorgonia Flabellum*.

The quantity of *silica* appears, as might be expected, to be in proportion to the density of the fibres of the sponge.

Shortly afterwards, on looking over Ellis's Zoophytes, p. 178, I found that he, in his description of *Gorgonia Briareus* (which is now considered to be an anomalous species), states, that its hard part (axis) or bone is composed of beautiful purple glassy *spiculae* lying lengthways almost parallel to each other."

After considerable search I have not been able to get or even see a specimen of this interesting species, but there can be very little doubt but that these *spiculae* are also siliceous.

This fact is exceedingly interesting in several points of view; first, because *silica* is very rarely found as a product of the animal kingdom, and has never hitherto, that I am aware of, been said to be found in the zoophytes, but only spoken of as a constituent of hair and horn, to which the axis of the sponges and gorgoniae have some resemblance; secondly, as proving a considerable affinity or resemblance in chemical composition, as well as in external structure, to exist between the sea and freshwater

sponges, a fact which several naturalists, since the appearance of my former paper, have appeared to doubt; and lastly, which, is of much more consequence, it proves, a considerable affinity to exist between the *Sponges* (both the *marine* and *fluviatile*) and the *Gorgonia*, which latter are known to be the habitation and production of individuals belonging to the animal kingdom; and this greatly strengthens the idea of Ray, Lamarck, and others, that the sponges are true corals, nearly allied to *Anthipates* and *Gorgonia*, and not vegetables; nor anomalous animals, like the *Infusoria*.

## ARTICLE VI.

### *On the Red Colour of Crystallized Felspar.*

(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

It has often occurred to me as a peculiarity in crystallized felspar, that it exhibits a decided red colour, though analysis points out no substance in its composition to which that colour can be attributed; and that this colour, after exposure to a strong heat, entirely disappears, leaving a very pure colourless glass. It cannot be attributed to iron even if a minute quantity should exist in the felspar, because the colour of iron is not destructible in this manner; and if a few iron stains exist on the felspar, the same heat which destroys the red colour of the crystal only makes those stains stronger. Hence a question has suggested itself, are chemists justified in supposing, as they uniformly do, that the colour of a mineral may always be referred to some specific colouring ingredient? It appears to me that felspar is an instance to the contrary, and an accidental experiment has enabled me to show that substances may be produced, which, though composed of perfectly colourless materials, shall, under certain circumstances, exhibit a decided colour. If a chemist should undertake to analyze the substance (of which I now send specimens), he would find only lime, alumina, silica, soda, and boracic acid, and he would be much puzzled to account for the red colour which it exhibits, particularly when he should find that the colour might be made to appear and disappear at pleasure, according to the degree of heat and of comminution to which he might expose it.

The method of producing the substance was as follows:—The ingredients above indicated were coarsely mixed together and exposed to a strong white heat, which produced a semi-vitrified mass of a pure white; a portion of this was finely ground, and after exposure to a low red heat not above that of melting silver, was found, much to my surprise, to have assumed

a red colour; which colour, with the increase of heat, was found entirely to disappear, and the substance assumed at last as pure a white, as it possessed after the first fusion. A lump of the original mass was found to undergo no change of colour at the same heat, and I have uniformly found, after several trials, that the depth of the colour depends on the fineness of the grinding. A portion was mixed with nitre, which almost entirely destroyed the colour, a proof that it cannot be attributed to manganese, which might perhaps be suspected, as nitre always deepens the colour of manganese in a remarkable manner.

I observe some doubts were expressed in your January number as to the nature of some specimens from Caernarvon: for my own part, as soon as I found that parts of the rock naturally of a red colour became white in strong fire, I had no doubt those parts consisted of felspar.

— . —

No. 1 is the result of the first fusion, the materials having been imperfectly mixed, it is porous, and not uniform, but of a good white.

No. 2 is the same substance finely ground in a porcelain dish.

No. 3 is a portion of No. 2, which has been exposed to a low red heat, nearly that of melting silver.\*

No. 4 is another portion of No. 2, which has been exposed to a heat somewhat stronger.†

No. 5 is another portion of No. 2, which has been exposed to a moderate white heat.‡

No. 6 is another portion mixed with one-fifth nitre, and exposed to the same heat as No. 3.§ C. C. C.

.



\* \* The phenomena related in our correspondent's paper are probably merely optical, owing to the different action of the substances on light from their different states of aggregation, according to the degrees of heat to which they have been exposed.—*Edit.*

\* This specimen has a peach blossom colour.

† Pale bluish lilac colour.

‡ Slightly greenish white enamel.

§ Similar tint to No. 4, but lighter.

## ARTICLE VII.

*Explanation of the Theory of the Barometrical Measurement of Heights.*(To the Editors of the *Annals of Philosophy*.)

GENTLEMEN,

THE barometer is an economical instrument capable, even in the hands of the most unscientific, of readily furnishing data sufficiently exact for the computation of accessible heights. The consequent calculations, in spite of repeated sacrifices of accuracy to dispatch, are however disgustingly tedious, and not a little liable to error. In the most approved formulæ, approximations seriously affecting in many cases the accuracy of the result are admitted, whilst minor corrections strictly constituting part of the value of the coefficients are unnecessarily kept distinct, and form a notable portion of the labour of the computer. The tables expressly constructed to facilitate and ensure accuracy to the nicer calculations of the philosopher, as well as those designed to abridge the labour to the geologist, the botanist, and the general traveller, for whom the *approximate* height may be sufficient, are capable of valuable improvement, not only in regard to accuracy, but to the attainment of the other object in view. To point out these errors, to remedy the defects, and to render the theory of the instrument and the various formulæ intelligible to general capacities, will form the principal object of the present paper. The task imposed is sufficiently difficult; the execution will therefore require the extreme indulgence of your readers.

Having formed a correct idea of the theory, we shall be able to propose some alterations in the construction of the instruments, trifling in themselves, yet enabling the observer materially to reduce the number of the data requisite for the calculations without affecting in the slightest degree the correctness of the result.

*Definition of Difference of Level, Vertical Height, &c.*

1. The earth being a sphere at rest, any two or other number of points equally distant from its centre are termed level points, —on a level,—or level with each other. 2. A level surface consists of such points, and is every where at the same distance from the centre of the earth. The level surface being of inconsiderable extent will be *sensibly* a plane, parallel to the horizon, and is consequently occasionally termed a horizontal surface or plane. 3. The difference of level, or the vertical height, elevation, or altitude of one point, or of one level surface above

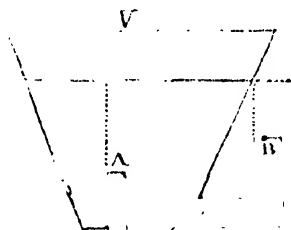
another, is equal to the difference of their distances from the centre of the earth.

### Of the Pressure of Fluids.

4. Fluids gravitate in lines directed to the centre of the earth (also that of gravity), and are so constituted that their particles yield to the action of the slightest pressure in any direction.

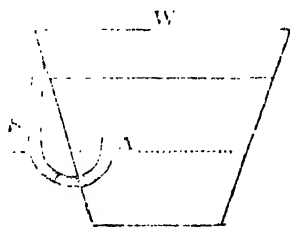
5. Every point of the surface of a fluid when at rest is equally distant from the centre of the earth.

6. The pressure *downwards* on the horizontal plane A (or *upwards* against the similar plane B) in contact with the uniformly dense fluid contained in the vessel V (placed in a vacuum), will vary directly as the vertical height of the surface of the fluid above the plane A (or the plane B), without regard to the figure or volume of the fluid; for the pressures are as the weights, and the weights are as the heights of the incumbent columns of the fluid.



7. The pressure depending *solely* on the vertical height of the fluid *above* the planes, without regard to its horizontal extent, depth *below* the planes, &c. the surface of the mercury (or other uniformly dense fluid) contained in an inverted syphon, will be level (or stand at the same height) in both branches, however different or irregular their diameters, and without regard to the degree of inclination of either. (The tubes are supposed to be sufficiently wide to render the effect of capillary attraction insensible.)

8. The pressure *downwards* within the shorter branch of the inverted syphon S, exerted on the surface A of the mercury (or other uniformly dense fluid) therein, in contact with the fluid contained in the vessel W (placed in a vacuum), will be equal to the weight of a cylinder C, having its base of the same area as the horizontal surface A; of the same vertical height as that of the surface of the fluid above A, and *uniformly* of the *mean* specific gravity of that intercepted portion of the fluid.



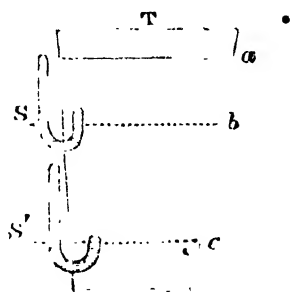
9. The *equiponderant* uniformly dense cylinder D, representing the weight of the counterpressure *upwards* of the mercury in the syphon S, will have a base equal to that of the cylinder C; its

height will be equal to the difference of level of the two branches; its specific gravity the same as that of the mercury.

10. Any ratio of the height of a cylindrical column uniformly dense is the same proportion of its weight, and of its volume.

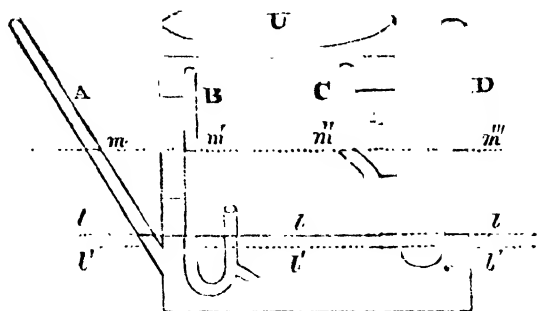
11. The diameters and weights of the two cylinders being equal, their heights will be reciprocally as their specific gravities. The *mean* specific gravity of the column of the fluid in the vessel *W*, intercepted between its surface and *A*, being less than that of the mercury in the syphon, in the ratio of 20 to 1, the vertical height of the fluid above *A* will exceed that of the difference of level of the mercury in the same proportion.

12. To ascertain the vertical height of the section *b c* situated below the surface *a* of the fluid contained in the vessel *T*; note the heights (differences of level) of the mercury in the syphons *S* and *S'*, placed at *b* and *c*. Conceiving the fluid *above* the level of *b* to be removed, the *difference* of these heights, in addition to the *mean* specific gravity of the intercepted fluid and that of the mercury, form the whole of the requisite data.



### Theory of the Barometer.

13. Mercury (water, or other heavy incompressible fluid) being poured into the vessel *U*, will ascend, and finally stand at the



same vertical height, or level *l l*, as well in the tube *C*, and in the inverted syphon *B* fixed within it as in the vessel itself, and in the erect tube *D*, and the inclined one *A* forming part of it.

14. The whole being in a vacuum, if we introduce into the

vessel a quantity of any fluid specifically lighter than the mercury, the resulting additional pressure *downwards* will give rise to a corresponding pressure *upwards*, and consequent ascent of the mercury within such parts of the vessel D and A, as well as within the tube C, and the closed branch of the syphon B, to which the rarer fluid has not access, and upon whose upper surfaces it cannot exert any part of the pressure incumbent on the mercury in the body of the vessel. The equilibrium of pressure will have taken place when a cylindrical column of the mercury, equal in height to the (vertical) difference of level of that fluid in any one of the closed tubes (their heights  $m, m', m'',$  and  $m'''$ , being the same), and in the body of the vessel (or open branch of the syphon) form an *exact counterpoise* to a similar column of the superincumbent fluid of the same base, and of its total height above the (reduced) level  $V, V'$  (or surface of contact of the two fluids).

*Remark.*—The subsidence of the level of the mercury in the vessel from  $U$  to  $V, V'$ , is occasioned by that part of its original volume having ascended, on the introduction of the superior fluid, into the closed tubes A, B, C, D.

Conceiving the light fluid pressing upon the exposed surface of the mercury to represent the atmosphere, then will the vessel U with its various tubes form so many varieties of the barometer having one common cistern.

15. Whatever the construction of the barometer, the *vertical* height of *that* surface of the mercury to which the atmosphere has not access *above* the surface in contact with it, equals, and is termed the pressure of atmosphere, or more generally the height of the barometer.

#### *Of the Density of Dry Air.*

*Variation from Pressure.*—16. A volume of perfectly dry air of any temperature, contained in the cylindrical vessel C, supporting only the pressure of the gravitating uniformly dense cylindrical weight W, will have its volume and height *diminished* in proportion to the *augmentation* of the weight (height) of the compressing column.

17. The weight (height) of the superincumbent cylinder W being *diminished* in any ratio, the volume (height of the column) of air will be *increased* in the same proportion.

18. Hence the elasticities of perfectly dry air are *directly*, and the volumes and heights *reciprocally* as the pressures (or heights of the compressing weights).

19. The volume occupied by any fluid being increased or diminished in any ratio, its density will be altered *inversely* in the same proportion. The densities will consequently be

directly as the pressures, and reciprocally as the volumes or heights.

*Variation from Temperature.*—20. The dry air, uniformly of the temperature of  $32^{\circ}$  F. contained in the cylinder C being exposed to increased temperatures, will have its volume, elasticity, and height, augmented, without regard to the pressure it supports at the uniform rate of  $\frac{1}{273}$  per degree. 21. Diminution of temperature will occasion a corresponding decrease of volume, elasticity, and height, in the same ratio.

#### Examples.

Temperatures . . . . .	$+32^{\circ}$ F.	$+ 89$	$+ 80^{\circ}$	$+ 512^{\circ}$
Volumes and heights	1.00000	0.95000	1.10000	2.00000
Densities . . . . .	1.00000	1.05263	0.90909	0.50000

Mr. Daniell has adopted, in his barometrical tables, &c. a most erroneous method of calculating the alterations of density from variation of temperature. Calling the volume of dry air under a given pressure (30 inches), and of the temperature of  $32^{\circ}$  F. = 1.0, he proceeds to find the densities at other (more elevated) temperatures by *subtracting* the corresponding increase of volume from 1.0, the assumed density at  $32^{\circ}$ . Had Mr. Daniell extended the table to  $512^{\circ}$ , at which temperature the original volume becomes doubled, the incorrectness of the method would have been detected,—the density would have come out 0! The heights computed from his table will consequently exceed the truth, especially when the mean temperature of the air was high, or the elevation of the mountain considerable. The altitude being 5000 feet, the error at  $80^{\circ}$  F. would be about 50 feet. Well might the author of the *Traité de Physique* make the remark, . . . . . “semblables à un riche malaisé qui n’a point d’ordre, au milieu de nos théories les plus brillantes, nous manquons souvent du plus simple nécessaire.”

(To be continued.)

## ARTICLE VIII.

*Alphabetical Table of the Weights of Atoms, according to Berzelius, corresponding with Phillips's Table, Annals of Philosophy, vol. xxiv. p. 185 (vol. vii. New Series).*

	Formulae.	Weights.	
Acid, acetic .....	A	611.12	
arsenic. ....	As	1440.77	
arsenious. ....	As	1240.77	
benzoic. ....	B	1509.55	
boracic. ....	B	269.65	
carbonic. ....	C	275.33	
chloric. ....	M	942.65	( <i>Acidum oxy muriaticum.</i> )
chronic. ....	Ch	1303.64	
citric. ....	C	727.85	
columbic. ....	Ta	1923.15	( <i>Acidum Tantalicum.</i> )
fluoboric. ....	F B	544.68	
fluoric. ....	F	275.03	
formic. ....	F	463.93	
fluosilicic. ....	F Si	2017.93	
gallic. ....	G	791.78	
iodic. ....	I	2066.70	( <i>Acidum oxiodicum.</i> )
molybdic. ....	Mo	896.80	
molybdous. ....	Mo	796.80	
muriatic. ....	M	342.65	{ (Considered as composed of the muriatic radicle = 142.65, and two atoms of oxygen.)
nitric. ....	N	677.26	
nitrous. ....	N	477.26	
oxalic. ....	O	2710.6	
perchloric. ....	M	1142.65	(As deduced from the composition of <i>Hyperoxymurias kalicus.</i> )
phosphoric. ....	P	892.30	
phosphorous. ....	P	692.30	
succinic. ....	S	627.85	

Formulae. [Weights.]

Acid, sulphuric.		501.16	
sulphurous.		401.16	
tartaric . . . .	T	834.49	
tungstic . . . .	W	1507.69	( <i>Acidum Wolframicum</i> ).
Alumina . . . . .	Al	642.33	}
sulphate . . . . .	Al S <sup>3</sup>	2145.80	
Aluminium . . . . .	Al	342.33	
Ammonia . . . . .	NH <sup>6</sup>	214.67	
acetate . . . . .	NH <sup>6</sup> A	856.56	
bicarbonate.	NH <sup>6</sup> C <sup>1</sup>	766.10	
borate . . . . .	NH <sup>6</sup> B	485.09	
carbonate . . . .	NH <sup>6</sup> C	490.77	
citrate . . . . .	NH <sup>6</sup> C	943.33	
iodate . . . . .	NH <sup>6</sup> I	2282.14	( <i>Oxiodas ammoniacus</i> .)
molybdate . . . .	NH <sup>6</sup> Mo	1112.24	
muriate . . . . .	NH <sup>6</sup> M	558.09	
nitrate . . . . .	NH <sup>6</sup> N	891.83	
oxalate . . . . .	NH <sup>6</sup> O	667.21	
phosphate . . . .	2 NH <sup>6</sup> + P	1321.44	
phosphite . . . .	2 NH <sup>6</sup> + P	1121.44	
succinate . . . . .	NH <sup>6</sup> S	842.42	
sulphate . . . . .	NH <sup>6</sup> S	715.73	
sulphite . . . . .	NH <sup>6</sup> S	615.73	
tartrate . . . . .	NH <sup>6</sup> T	1049.06	
Antimony . . . . .	Sb	1612.90	( <i>Stibium</i> .)
chloride . . . . .	Sb M <sup>3</sup>	2940.85	
iodide . . . . .	Sb I <sup>3</sup>	6313.0	
deutoxide . . . . .	Sb	2012.90	( <i>Acidum stibiosum?</i> )
peroxide . . . . .	Sb	2112.90	( <i>Acidum Stibicum?</i> )
protoxide . . . . .	Sb	1912.90	
sulphuret . . . . .	Sb S <sup>3</sup>	2216.38	

	Formulae.	Weights.	
Arsenate of ammonia . . .	$2 \text{ NH}_4^+ \text{ AsO}_4^{3-}$	1871.65	
potash . . . . .	$\text{K AsO}_4$	2620.60	
soda . . . . .	$\text{Na AsO}_4$	2222.61	
Arsenic . . . . .	$\text{As}$	940.77	
Azote . . . . .	$\text{N}$	177.26	} ((Considered as a protoxide of <i>nitricum</i> , the imaginary base of <i>azote</i> , whose number is 77.26.)
Barium . . . . .	$\text{Ba}$	1713.86	
chloride . . . . .	$\text{Ba Cl}_2$	2599.16	
iodide . . . . .	$\text{Ba I}_2$	4847.26	
peroxide . . . . .	$\text{Ba O}_2$	2013.86	
phosphuret . . . . .	$\text{Ba P}$	2106.16	
Barytes . . . . .	$\text{Ba}$	1913.86	
acetate . . . . .	$\text{Ba Ac}$	3196.1	
arsenate . . . . .	$\text{Ba AsO}_4$	3354.63	
arsenite . . . . .	$\text{Ba AsO}_3$	4395.40	
benzoate . . . . .	$\text{Ba Bz}$	4932.96	
borate . . . . .	$\text{Ba B}$	2453.17	
carbonate . . . . .	$\text{Ba C}$	2464.52	
chlorate . . . . .	$\text{Ba ClO}_3$	3799.16	
chromate . . . . .	$\text{Ba CrO}_4$	3217.50	
citrate . . . . .	$\text{Ba C}_3$	3369.56	
hydrate . . . . .	$\text{Ba} + 2 \text{ Aq}$	2138.73	
iodate . . . . .	$\text{Ba IO}_3$	6047.26	
nitrate . . . . .	$\text{Ba N}_3$	3268.38	
muriate (cryst. 1 } water) . . . . . }	$\text{Ba M} + 4 \text{ Aq}$	3048.90	
oxalate . . . . .	$\text{Ba O}_2$	2817.40	
phosphate . . . . .	$\text{Ba P}$	2806.16	
phosphite . . . . .	$\text{Ba P}$	2606.16	
succinate . . . . .	$\text{Ba S}_2$	3169.56	
sulphate . . . . .	$\text{Ba S}_2$	2916.18	
sulphite . . . . .	$\text{Ba S}$	2716.13	

Formulæ. |Weights.|

Barytes, tartrate. ....	Ba T <sup>a</sup>	3582·84	
tungstate .....	Ba W <sup>a</sup>	4929·24	<i>Wolframias baryticus.</i> )
Benzoic acid .	B	1509·55	
Bicarburetted hydrogen. H <sup>a</sup> C		88·60	{ <i>Carburetum Hydrogenii</i> , a more correct name than the one we use in this country.)
Bismuth .....	Bi	1773·8	
acetate ....	Bi A <sup>a</sup>	3256·0	
arsenate ..	Bi As	3414·57	
benzoate ..	Bi B <sup>a</sup>	4992·90	
chloride ...	Bi M <sup>a</sup>	2659·10	
citrate ....	Bi C <sup>a</sup>	3429·5	
iodate .....	Bi I <sup>a</sup>	6107·2	
iodide .....	Bi I <sup>a</sup>	4907·2	
nitrate ....	Bi N <sup>a</sup>	3328·32	
oxalate ....	Bi O <sup>a</sup>	2877·34	
oxide .....	Bi	1973·80	
phosphate .	Bi P <sup>a</sup>	2866·10	
sulphate ..	Bi S <sup>a</sup>	2976·12	
sulphuret ..	Bi S <sup>a</sup>	2176·12	
tartrate ...	Bi T <sup>a</sup>	3642·78	
Boracic acid ...	B	269·65	
Boron .....	B	69·655	
Cadmium .....	Cd	1393·54	As deduced from its salts. See also <i>Essai</i> , p. 145.)
carbonate ..	C <sup>d</sup> C <sup>a</sup>	2144·20	
chloride ...	C <sup>d</sup> M <sup>a</sup>	2278·84	
iodide .....	Cd I <sup>a</sup>	4526·94	
nitrate ....	C <sup>d</sup> N <sup>a</sup>	2948·06	
oxide .....	C <sup>d</sup>	1593·54	
phosphate .	C <sup>d</sup> P <sup>a</sup>	2485·84	
sulphate ..	C <sup>d</sup> S <sup>a</sup>	2595·86	
sulphuret ..	C <sup>d</sup> S <sup>a</sup>	1795·86	

	Formulae.	Weights.
Calcium .....	$\text{Ca}$	512.06
chloride .....	$\text{Ca M}^2$	1397.36
fluoride .....	$\text{Ca F}$	987.09
iodide .....	$\text{Ca I}^2$	3645.46
oxide (lime) .....	$\text{Ca}$	712.06
phosphuret .....	$\text{Ca P}^2$	904.36
Calomel .....	$\text{H M}$	2974.25
Carbon .....	$\text{C}$	75.33
oxide .....	$\text{C}$	175.33
sulphuret .....	$\text{C S}^2$	477.65
Carbonic acid .....	$\text{C}$	275.33
Carburetted hydrogen ..	$\text{H}^2 \text{C}$	101.86 ( <i>Carburetum bihydrogenicum</i> )
Cerium .....	$\text{Ce}$	1149.44
Chlorine .....	$\text{M}$	221.325 ( <i>Radicle of muriatic acid = 142.65.</i> See <i>Essai</i> , p. 125.)
Chromium .....	$\text{Ch}$	703.64
dentoxide .....	$\text{Ch}$	1103.64
oxide .....	$\text{Ch}$	1003.64
Cobalt .....	$\text{Co}$	738.00
acetate .....	$\text{Co A}^2$	2220.2
arseniate .....	$\text{Co As}$	2378.77
benzoate .....	$\text{Co B}^2$	3957.10
borate .....	$\text{Co B}^2$	1477.31
carbonate .....	$\text{Co C}^2$	1488.66
chloride .....	$\text{Co M}$	1623.3
citrate .....	$\text{Co C}^2$	2393.7
iodide .....	$\text{Co I}^2$	3871.4
nitrate .....	$\text{Co N}^2$	2292.52
oxalate .....	$\text{Co O}^2$	1841.54
peroxide .....	$\text{Co}$	1038.00
phosphate .....	$\text{Co P}$	1230.30

	Formulae.	Weights.	
Cobalt, protoxide.....	$\begin{array}{c} \cdot \\ \text{Co} \end{array}$	938.00	
sulphate ...	$\begin{array}{c} \cdot \\ \text{Co S}^2 \end{array}$	1940.32	
sulphuret. . .	$\begin{array}{c} \cdot \\ \text{Co S}^2 \end{array}$	1140.32	
tartrate ....	$\begin{array}{c} \cdot \\ \text{Co T}^2 \end{array}$	2606.98	
Columbium. ....	Ta	1823.15	<i>Tantalum</i> , a name the metal has no right to.)
Copper. ....	$\begin{array}{c} \cdot \\ \text{Cu} \end{array}$	791.39	
acetate. ....	$\begin{array}{c} \cdot \\ \text{Cu A}^2 \end{array}$	2273.6	
carbonate. . .	$\begin{array}{c} \cdot \\ \text{Cu C}^2 \end{array}$	1542.05	
iodide.....	$\begin{array}{c} \cdot \\ \text{Cu I}^2 \end{array}$	3924.79	
perchloride .	$\begin{array}{c} \cdot \\ \text{Cu M}^2 \end{array}$	1676.69	
pernitrate ..	$\begin{array}{c} \cdot \\ \text{Cu N}^2 \end{array}$	2345.91	
persulphate.	$\begin{array}{c} \cdot \\ \text{Cu S}^2 \end{array}$	1993.71	
perphosphate.....	$\begin{array}{c} \cdot \\ \text{Cu P}^2 \end{array}$	1883.69	
protochloride .....	$\begin{array}{c} \cdot \\ \text{Cu M} \end{array}$	1234.04	
protoxide. .	$\begin{array}{c} \cdot \\ \text{Cu} \end{array}$	891.39	
peroxide . .	$\begin{array}{c} \cdot \\ \text{Cu} \end{array}$	991.39	
sulphuret. .	$\begin{array}{c} \cdot \\ \text{Cu S} \end{array}$	992.53	
Fluorine.....	Fl	75.03	
Glucina .....	$\begin{array}{c} \cdot \\ \text{Be} \end{array}$	962.56	<i>Oxidium beryllicum.</i> )
Glucinum .....	$\begin{array}{c} \cdot \\ \text{Be} \end{array}$	662.56	<i>Beryllium.</i> )
Gold. ....	$\begin{array}{c} \cdot \\ \text{Au} \end{array}$	2486.00	
chloride .....	$\begin{array}{c} \cdot \\ \text{Au M}^2 \end{array}$	3813.95	
protoxide. . .	$\begin{array}{c} \cdot \\ \text{Au} \end{array}$	2586.00	
peroxide . . .	$\begin{array}{c} \cdot \\ \text{Au} \end{array}$	2786.00	
sulphuret. . .	$\begin{array}{c} \cdot \\ \text{Au S}^2 \end{array}$	3089.48	
Hydrogen .....	H	6.2177	
Iodine. ....	I	1266.7	
Iron. . ....	Fe	678.43	
protechloride.	$\begin{array}{c} \cdot \\ \text{Fe M}^2 \end{array}$	1563.73	
perchloride. ....	$\begin{array}{c} \cdot \\ \text{Fe M} \end{array}$	2006.38	

## Formulæ. [Weights.

Iron, peroxide. ....		978.43	
protoxide. ....		878.43	
sulphate. ....	$\text{Fe S}^4$	2481.91	
persulphuret. ....	$\text{Fe S}^4$	183.07	
protosulphuret. ...	$\text{Fe S}^2$	980.75	
Lead. ....	$\text{Pb}$	89.00	
acetate. ....	$\text{Pb A}^2$	71.2	
sub-tritacetate. ...	$\text{Pb}^3 \text{A}^2$	149.2	( <i>Acetas triplumbicus.</i> )
arsenate. ....	$\text{Pb As}$	229.77	
benzoate. ....	$\text{Pb B}^3$	1838.10	
borate. ....	$\text{Pb B}^3$	1328.31	
carbonate. ....	$\text{Pb C}^2$	339.33	
chloride. ....	$\text{Pb M}^2$	1474.3	
chromate. ....	$\text{Pb Ch}$	992.64	
citrate. ....	$\text{Pb C}^2$	1244.70	
deutoxide. ....	$\text{Pb}$	889.00	( <i>Superoxidum plumbosum.</i> )
iodate. ....	$\text{Pb I}^3$	1722.4	
iodide. ....	$\text{Pb I}^2$	1722.4	
molybdate. ....	$\text{Pb Mo}^4$	582.6	
nitrate. ....	$\text{Pb N}^3$	1143.52	
oxalate. ....	$\text{Pb O}^2$	3692.54	
peroxide. ....	$\text{Pb}$	2989.00	( <i>Superoxidum plumbicum.</i> )
phosphate. ....	$\text{Pb P}$	3681.30	
phosphite. ....	$\text{Pb P}$	3481.30	
protoxide. ....	$\text{Pb}$	2789.00	( <i>Oxidum plumbicum.</i> )
succinate. ....	$\text{Pb S}^2$	4044.70	
sulphate. ....	$\text{Pb S}^4$	3791.32	
sulphite. ....	$\text{Pb S}^3$	3591.32	
sulphuret. ....	$\text{Pb S}^2$	2991.32	
tartrate. ...	$\text{Pb T}^2$	4457.98	

	Formulae.	Weights.
Lime. ....	$\ddot{\text{Ca}}$	712.06
acetate. ....	$\ddot{\text{Ca}} \overset{\text{---}}{\text{A}^2}$	1994.3
arsenate. ....	$\ddot{\text{Ca}} \overset{\text{---}}{\text{As}}$	2152.83
benzoate. ....	$\ddot{\text{Ca}} \overset{\text{---}}{\text{B}^2}$	3731.16
biphosphate. ....	$\ddot{\text{Ca}} \overset{\text{---}}{\text{P}^2}$	2496.66
borate. ....	$\ddot{\text{Ca}} \overset{\text{---}}{\text{B}^2}$	1251.37
carbonate. ....	$\ddot{\text{Ca}} \overset{\text{---}}{\text{C}^2}$	1262.72
chlorate. ....	$\ddot{\text{Ca}} \overset{\text{---}}{\text{M}^2}$	2597.36
chloride. ....	$\ddot{\text{Ca}} \overset{\text{---}}{\text{M}^2}$	1397.36
citrate. ....	$\ddot{\text{Ca}} \overset{\text{---}}{\text{C}^2}$	2167.76
chromate. ....	$\ddot{\text{Ca}} \overset{\text{---}}{\text{Ch}}$	2015.70
hydrate. ....	$\ddot{\text{Ca}} + 2 \text{Aq}$	936.93
iodate. ....	$\ddot{\text{Ca}} \overset{\text{---}}{\text{I}^2}$	4845.46
oxalate. ....	$\ddot{\text{Ca}} \overset{\text{---}}{\text{O}^2}$	1615.58
phosphate. ....	$\ddot{\text{Ca}} \overset{\text{---}}{\text{P}}$	1604.36
phosphite. ....	$\ddot{\text{Ca}} \overset{\text{---}}{\text{P}}$	1404.36
succinate. ....	$\ddot{\text{Ca}} \overset{\text{---}}{\text{S}^2}$	1967.76
sulphate. ....	$\ddot{\text{Ca}} \overset{\text{---}}{\text{S}^2}$	1714.39
sulphite. ....	$\ddot{\text{Ca}} \overset{\text{---}}{\text{S}^2}$	1514.38
tartrate. ....	$\ddot{\text{Ca}} \overset{\text{---}}{\text{T}^2}$	2381.04
tungstate. ....	$\ddot{\text{Ca}} \overset{\text{---}}{\text{W}^2}$	3727.44 ( <i>Wolframias calcicus.</i> )
Lithia. ....	$\ddot{\text{L}}$	455.63
carbonate. ....	$\ddot{\text{L}} \overset{\text{---}}{\text{C}^2}$	1006.29
nitrate. ....	$\ddot{\text{L}} \overset{\text{---}}{\text{N}^2}$	1810.15
phosphate. ....	$\ddot{\text{L}} \overset{\text{---}}{\text{P}}$	1347.93
sulphate. ....	$\ddot{\text{L}} \overset{\text{---}}{\text{S}^2}$	1457.95
Lithium. ....	$\ddot{\text{L}}$	255.63
chloride. ....	$\ddot{\text{L}} \overset{\text{---}}{\text{M}^2}$	1140.93
iodide. ....	$\ddot{\text{L}} \overset{\text{---}}{\text{I}^2}$	3389.03
Magnesia. ....	$\ddot{\text{Mg}}$	516.72

	Formulae.	Weights.
Magnesia, borate . . . .	$\text{Mg B}$	056.08
carbonate . . . . .	$\text{Mg C}^2$	067.38
hydrate . . . . .	$\text{Mg} + 2 \text{Aq}$	741.59
nitrate . . . . .	$\text{Mg N}^2$	871.24
phosphate . . . . .	$\text{Mg P}$	409.02
sulphate . . . . .	$\text{Mg S}^2$	519.04
tartrate . . . . .	$\text{Mg T}^2$	2185.70
Magnesium . . . . .	$\text{Mg}$	316.72
chloride . . . . .	$\text{Mg M}^2$	202.02
iodide . . . . .	$\text{Mg I}^2$	3450.12
Manganese . . . . .	$\text{Mn}$	711.57
acetate . . . . .	$\text{Mn A}^3$	2935.0
benzoate . . . . .	$\text{Mn B}^3$	540.22
carbonate . . . . .	$\text{Mn C}^2$	1462.33
chlorate . . . . .	$\text{Mn M}^2$	2198.87
chloride . . . . .	$\text{Mn M}^2$	1596.87
citrate . . . . .	$\text{Mn C}^4$	2467.27
deutoxide . . . . .	$\text{Mn}$	1011.57 ( <i>Oxidum manganicum.</i> )
oxalate . . . . .	$\text{Mn O}^3$	2366.88
peroxide . . . . .	$\text{Mn}$	1111.37 ( <i>Superoxidum manganicum.</i> )
phosphate . . . . .	$\text{Mn}^2 \text{P}^3$	4700.04
protoxide . . . . .	$\text{Mn}$	911.57 ( <i>Oxidum manganosum.</i> )
succinate . . . . .	$\text{Mn S}^3$	2895.12
sulphate . . . . .	$\text{Mn S}^2$	2515.05
tartrate . . . . .	$\text{Mn T}^2$	2580.55
Mercury . . . . .	$\text{Hg}$	2531.60 ( <i>Hydrargyrum.</i> )
bisulphuret . . . . .	$\text{Hg S}^2$	2933.92
perchloride . . . . .	$\text{Hg M}^2$	3416.9
periodide . . . . .	$\text{Hg I}^2$	5665.0
pernitrate . . . . .	$\text{Hg N}^4$	4086.12

Formulæ. |Weights.]

Mercury, peroxide. ....	$\text{Hg} \begin{smallmatrix} \cdot\cdot\cdot \\ \cdot\cdot\cdot \end{smallmatrix}$	2731.6	( <i>Oxidum Hydrargyricum.</i> )
perphosphate. . . .	$\text{Hg} \text{P} \begin{smallmatrix} \cdot\cdot\cdot \\ \cdot\cdot\cdot \end{smallmatrix}$	3623.9	
persulphate. ....	$\text{Hg} \text{S} \begin{smallmatrix} \cdot\cdot\cdot \\ \cdot\cdot\cdot \end{smallmatrix}$	3633.92	( <i>Sulphas hydrargyricus.</i> )
protochloride, .....	$\text{Hg} \text{M} \begin{smallmatrix} \cdot\cdot\cdot \\ \cdot\cdot\cdot \end{smallmatrix}$	2974.25	
protonitrate. ....	$\text{Hg} \text{N} \begin{smallmatrix} \cdot\cdot\cdot \\ \cdot\cdot\cdot \end{smallmatrix}$	3308.86	
protosulphate. ..	$\text{Hg} \text{S} \begin{smallmatrix} \cdot\cdot\cdot \\ \cdot\cdot\cdot \end{smallmatrix}$	3132.76	( <i>Sulphas hydrargyrosus.</i> )
protoxide. ....	$\text{Hg}$	2631.6	( <i>Oxidum Hydrargyrosus.</i> )
Molybdenum. ....	$\text{Mo}$	596.80	
protoxide. ....	$\text{Mo}$	696.80	
Nickel. ....	$\text{Ni}$	739.51	
acetate. ....	$\text{Ni} \text{A} \begin{smallmatrix} \cdot\cdot\cdot \\ \cdot\cdot\cdot \end{smallmatrix}$	2221.7	
arseniate. ....	$\text{Ni} \text{As} \begin{smallmatrix} \cdot\cdot\cdot \\ \cdot\cdot\cdot \end{smallmatrix}$	2380.28	
carbonate. ....	$\text{Ni} \text{C} \begin{smallmatrix} \cdot\cdot\cdot \\ \cdot\cdot\cdot \end{smallmatrix}$	1490.17	
chloride. ....	$\text{Ni} \text{M} \begin{smallmatrix} \cdot\cdot\cdot \\ \cdot\cdot\cdot \end{smallmatrix}$	1624.81	
citrate. ....	$\text{Ni} \text{C} \begin{smallmatrix} \cdot\cdot\cdot \\ \cdot\cdot\cdot \end{smallmatrix}$	2395.21	
iodide. ....	$\text{Ni} \text{I} \begin{smallmatrix} \cdot\cdot\cdot \\ \cdot\cdot\cdot \end{smallmatrix}$	3872.91	
nitrate. ....	$\text{Ni} \text{N} \begin{smallmatrix} \cdot\cdot\cdot \\ \cdot\cdot\cdot \end{smallmatrix}$	2294.03	
oxalate. ....	$\text{Ni} \text{O} \begin{smallmatrix} \cdot\cdot\cdot \\ \cdot\cdot\cdot \end{smallmatrix}$	1843.05	
peroxide. ....	$\text{Ni} ?$	1039.51	
phosphate. ....	$\text{Ni} \text{P} \begin{smallmatrix} \cdot\cdot\cdot \\ \cdot\cdot\cdot \end{smallmatrix}$	1831.81	
protoxide. ....	$\text{Ni}$	939.51	
sulphate. ....	$\text{Ni} \text{S} \begin{smallmatrix} \cdot\cdot\cdot \\ \cdot\cdot\cdot \end{smallmatrix}$	1941.83	
sulphuret. ....	$\text{Ni} \text{S} \begin{smallmatrix} \cdot\cdot\cdot \\ \cdot\cdot\cdot \end{smallmatrix}$	1141.83	
tartrate. ....	$\text{Ni} \text{T} \begin{smallmatrix} \cdot\cdot\cdot \\ \cdot\cdot\cdot \end{smallmatrix}$	2608.49	
Nitric oxide. ....	$\text{N}$	377.26	( <i>Oxidum nitricum—gas nitrosum.</i> )
Nitrogen. ....	$\text{N}$	177.26	( <i>Suboxidum nitricum ; see Azote.</i> )
Nitrous oxide. ....	$\text{N}$	277.26	( <i>Oxidum nitrosum.</i> )
Osmium. ....	$\text{Os} ?$		
oxide. ....	$?$		
Oxygen. ....	$\text{O}$	100.00	

Formula. Weights.

Palladium .....	$\text{Pa}$	407.50	
oxide .....	$\text{Pa}$	1607.50	
Phosphorus. ....	$\text{P}$	392.30	
Platinum .....	$\text{Pt}$	1215.23	
chloride ....	$\text{Pt M}$	1651.88	( <i>Musius platinumus.</i> )
perchloride ..	$\text{Pt M}^2$	2100.53	
peroxide. ....	$\text{Pt}$	1415.23	
protoxide. ....	$\text{Pt}$	1315.23	
Potash .....	$\text{K}$	1179.83	( <i>Kali.</i> )
acetate .....	$\text{K A}^2$	2162.0	
arsenate ..	$\text{K As}$	2690.60	
arsenite ....	$\text{K As}^2$	3661.31	
benzoate. ....	$\text{K a B}^2$	1198.93	
bicarbonate. ....	$\text{K C}^3$	2281.15	
binarsenate. ....	$\text{K As}^2$	4061.37	
binoxalate ..	$\text{K O}^4$	2986.91	
biphosphate. ....	$\text{K P}$	2964.43	
bisulphate .....	$\text{K S}^4$	3184.47	
bitartrate. ....	$\text{K T}^4$	4517.79	
borate. ....	$\text{K B}^3$	1719.14	
carbonate. ....	$\text{K C}^3$	1730.49	
chlorate .....	$\text{K M}^3$	3065.13	
chromate .....	$\text{K Ch}$	2483.47	
citrate. ....	$\text{K C}^3$	2635.5	
hydrate .....	$\text{K} + 2 \text{Aq}$	1404.70	
iodate .....	$\text{K I}^3$	5313.23	
molybdate .....	$\text{K Mo}^2$	2973.43	
nitrate .....	$\text{K N}^2$	2534.85	
oxalate. ....	$\text{K O}^2$	2083.37	
phosphate .....	$\text{K P}$	2072.13	

## Formulae. Weights.

Potash, quadroxalate.	$\text{K O}^6$	1793.99	
succinate . . .	$\text{K S}^3$	2435.53	
sulphate . . .	$\text{K S}^2$	182.15	
sulphite . . .	$\text{K S}^2$	1992.15	
tartrate . . .	$\text{T}^2$	2848.81	
tungstate . .	$\text{K W}^2$	1195.21	( <i>Wolframias kalicus</i> .)
Potassium . . . . .	$\text{K}$	979.83	<i>Kalium</i> .)
chloride . . .	$\text{K M}^2$	865.13	
iodide . . . .	$\text{K I}^2$	1113.23	
peroxide . .	$\text{K}$	1579.83	
phosphuret	$\text{K P}^2$	1372.13	
protioxide . .		1179.83	{ Berzelius gives another oxide of potassium, viz. <i>Suboxidum kalicum</i> . $\text{K} = 1079.83$ .)
sulphuret . .	$\text{K S}^2$	382.15	
Rhodium . . . . .	$\text{R}$	1500.10	
peroxide . .	$\text{R}$	1800.10	{ ( <i>Oxidum rhodicum</i> . According to Berzelius there is a deutoxide, <i>Oxidum Rhodcum</i> , $\text{R} = 1700.10$ .)
protioxide . .	$\text{R}$	1600.10	
Selenium . . . . .	$\text{Se}$	495.91	
Silica . . . . .		596.42	
Silicium . . . . .		296.42	
Silver . . . . .	$\text{Ag}$	2703.21	
acetate . .	$\text{Ag A}^2$	4185.45	
arsenate . .	$\text{Ag As}$	4343.98	
arsenite . .	$\text{Ag As}^2$	5384.75	
benzoate . .	$\text{Ag B}^2$	5922.31	
borate . . .	$\text{Ag B}^2$	3442.52	
carbonate . .	$\text{Ag C}^2$	3453.87	
chlorate . .	$\text{Ag M}^2$	4788.51	
chloride . .	$\text{Ag M}^2$	3588.51	
chromate . .	$\text{Ag Ch}$	4206.85	

	Formule.	Weights.
Silver, citrate .....	Ag C <sup>3</sup>	1358.91
iodate .....	Ag I <sup>2</sup>	7036.61
iodide .....	Ag I <sup>1</sup>	5836.61
molybdate .....	Ag Mo <sup>2</sup>	4696.81
nitrate .....	Ag N <sup>3</sup>	1257.73
oxalate .....	Ag O <sup>2</sup>	3806.75
oxide .....	Ag	2903.21
phosphate .....	Ag P	3795.51
sulphate .....	Ag S <sup>2</sup>	3905.53
sulphite .....	Ag S <sup>2</sup>	3705.53
sulphuret .....	Ag S <sup>2</sup>	3105.53
tartrate .....	Ag T <sup>2</sup>	4572.19
tungstate .....	Ag W <sup>2</sup>	5918.59
Soda .....	Na	781.84 ( <i>Orichum nativum.</i> )
acetate .....	Na A <sup>2</sup>	2064.0
arsenate .....	Na As	2222.61
arsenite .....	Na As <sup>3</sup>	3263.38
benzoate .....	Na B <sup>2</sup>	3800.94
bicarbonate .....	Na Ca <sup>4</sup>	1883.16
borate .....	Na B <sup>2</sup>	1321.15
carbonate .....	Na C <sup>2</sup>	1332.50
chlorate .....	Na M <sup>2</sup>	2667.14
chromate .....	Na Ch	2055.48
citrate .....	Na C <sup>3</sup>	2237.54
hydrate .....	Na : 2 Ag	1006.71
iodate .....	Na I <sup>2</sup>	1915.24
molybdate .....	Na Mo <sup>2</sup>	2575.44
nitrate .....	Na N <sup>3</sup>	2136.36
oxalate .....	Na O <sup>2</sup>	1685.38
succinate .....	Na S <sup>2</sup>	2037.54
	2 G 2	

	Formulæ.	Weights.
Soda, sulphate.....	$\ddot{\text{Na}} \ddot{\text{S}}^2$	1784.16
sulphite.....	$\ddot{\text{Na}} \ddot{\text{S}}^2$	1584.16
tartrate.....	$\ddot{\text{Na}} \ddot{\text{T}}^2$	2450.80
Sodium.....	$\ddot{\text{Na}}$	581.84 ( <i>Natrium.</i> )
chloride.....	$\ddot{\text{Na}} \ddot{\text{M}}^2$	1467.14
iodide.....	$\ddot{\text{Na}} \ddot{\text{I}}^2$	3715.24
peroxide.....	$\ddot{\text{Na}}$	881.84
protoxide.....	$\ddot{\text{Na}}$	781.84
sulphuret.....	$\ddot{\text{Na}} \ddot{\text{S}}^2$	984.16
Strontia.....	$\ddot{\text{Sr}}$	1294.60
acetate.....	$\ddot{\text{Sr}} \ddot{\text{A}}^2$	2576.8
borate.....	$\ddot{\text{Sr}} \ddot{\text{B}}^3$	1833.91
carbonate.....	$\ddot{\text{Sr}} \ddot{\text{C}}^2$	1845.26
citrate.....	$\ddot{\text{Sr}} \ddot{\text{C}}^2$	2750.3
hydrate.....	$\ddot{\text{Sr}} + 2 \text{Aq}$	1521.13
oxalate.....	$\ddot{\text{Sr}} \ddot{\text{O}}^4$	2198.14
phosphate.....	$\ddot{\text{Sr}} \ddot{\text{P}}^3$	2186.90
sulphate.....	$\ddot{\text{Sr}} \ddot{\text{S}}^2$	2296.92
tartrate.....	$\ddot{\text{Sr}} \ddot{\text{T}}^2$	2963.58
Strontium.....	$\ddot{\text{Sr}}$	1094.60
chloride.....	$\ddot{\text{Sr}} \ddot{\text{M}}^2$	1979.91
iodide.....	$\ddot{\text{Sr}} \ddot{\text{I}}^2$	4228.0
Sulphur.....	$\ddot{\text{S}}$	201.16
carburet.....	$\ddot{\text{C}} \ddot{\text{S}}^2$	477.65 ( <i>Sulphuretum carbonici.</i> )
Sulphuretted hydrogen..	$\text{H}^2 \text{S}$	213.60
Tellurium.....	$\ddot{\text{Te}}$	806.45
chloride.....	$\ddot{\text{Te}} \ddot{\text{M}}^2$	1691.75
oxide.....	$\ddot{\text{Te}}$	1006.45
Tin.....	$\ddot{\text{Sn}}$	1470.58 ( <i>Stannum.</i> )
bisulphuret.....	$\ddot{\text{Sn}} \ddot{\text{S}}^4$	3275.22

(*Superoxidum natricum.* Berzelius also gives a suboxidum natri-  
cum, N = 681.84.)

## Formulae. Weights.

Tin, iodide .....	$\text{Sn I}^2$	4603.98	{ <i>Iodas stannosus.</i> The periodide, or <i>Iodas stannicus</i> , $\text{Sn I}^2 = 7737.38.$ }
peroxide.....	$\text{Sn}$	1870.58	
protoxide.....	$\text{Sn}$	1670.58	
perchloride.....	$\text{Sn M}^4$	3241.18	( <i>Murius stannicus.</i> )
protochloride.....	$\text{Sn M}^2$	2355.88	( <i>Murius stannosus.</i> )
sulphuret.....	$\text{Sn S}^2$	1872.90	
Titanium.....	$\text{Ti}$		
Tungsten.....	$\text{W}$	1207.69	( <i>Wolframium.</i> )
Tungstic acid .....	$\text{W}$	1507.69	( <i>Acidum Wolframicum.</i> )
Uranium.....	$\text{U}$	3146.86	
oxide .....	$\text{U}$	3346.86	( <i>Oxidum uranicum.</i> )
peroxide .....	$\text{U}$	3446.86	( <i>Oxidum uranicum.</i> )
Water.....	$\text{Aq}$	112.4354	
Yttria .....	$\text{Y}$	1005.14	
Yttrium.....	$\text{Y}$	805.14	
Zinc.....	$\text{Zn}$	806.45	
acetate...	$\text{Zn A}^2$	2288.7	
arsenate .....	$\text{Zn As}$	2447.22	
benzoate...	$\text{Zn B}^2$	4025.55	
borate.....	$\text{Zn B}^2$	1545.76	
carbonate...	$\text{Zn C}$	1557.11	
chlorate .....	$\text{Zn M}^2$	2891.75	
chloride .....	$\text{Zn M}^2$	1691.75	
citrate...	$\text{Zn C}^2$	2462.15	
iodate .....	$\text{Zn I}^2$	5139.85	
iodide .....	$\text{Zn I}^2$	3939.85	
nitrate.....	$\text{Zn N}^2$	2360.97	
oxalate...	$\text{Zn O}^2$	1909.99	
oxide .....	$\text{Zn}$	1006.45	
phosphate .....	$\text{Zn P}$	1898.75	

	Formulae.	Weights.
Zinc, succinate .....	$\ddot{\text{Zn}} \text{ S}^2$	2262.15
sulphate.....	$\ddot{\text{Zn}} \text{ S}^2$	2008.77
sulphite .....	$\ddot{\text{Zn}} \text{ S}^2$	1808.77
tartrate.....	$\ddot{\text{Zn}} \text{ T}^2$	2675.43
Zirconia.....	Zr Ox	?
Zirconium.....	Zr	?

## ARTICLE IX.

## ANALYSES OF BOOKS.

*On the Safety Lamp, for preventing Explosions in Mines, Houses lighted by Gas, Spirit Warehouses, or Magazines in Ships, &c. with some Researches on Flame.* By Sir Humphry Davy, Bart. President of the Royal Society, &c.

IT is an extraordinary fact, and one that does little credit either to the cultivators of science in general, or those persons who are interested in mining operations in particular, that whilst pseudo-scientific catechisms and dialogues, and such trash, find purchasers in every class of society, and enrich their empirical compilers, spite of the mass of error and absurdity with which nine times in ten they abound—it is extraordinary, we say, that whilst such abject things as those are read with avidity, a single edition of a work like this has not been wholly disposed of in seven years! To what this is owing, it is not our business to inquire; but we know that books of sterling merit often pass unnoticed by the multitude, who at the same time eagerly catch at every flimsy production which promises (what, by the bye, it never performs) to reduce the most abstruse philosophical reasoning to the level and comprehension of the meanest capacities.\* Thus we have astronomy, optics, and mechanics divested of the mathematics, chemistry illustrated by drawings without the necessity of apparatus or experiment, and crystallography and even the mathematical sciences themselves taught by dissected models and diagrams: and what is the consequence? Ignorance usurps the seats of wisdom—the unassuming diffidence of real knowledge is silenced and abashed before the clamorous dissertations of pretending quacks—and instead of the sublime truths of genuine philosophy, the minds of our youth are filled

\* We must exclude, however, from this class, Mrs. Marcet's admirable *Conversations*; but *abi ullum invenies parum?*

with false notions, and still false estimates of their own supposed powers and acquirements.

These remarks are but too applicable to the case before us. All the world has heard of Sir Humphry Davy's Safety Lamp, but how many of its members are acquainted with the principles on which it is founded? Here the ignorance is not merely absurd, but dangerous; for a want of that knowledge has led in many cases, and probably will lead in many more, to consequences disastrous and deplorable in the extreme. We have ourselves noticed in our walks about this great town, round wire cages, in many of the shop windows, miscalled safety lanthorns, the meshes of which are so large that a candle in a wire mouse-trap would be as good a security against accident in an explosive atmosphere as one of those precious safeguards; and yet real safety lamps may be purchased both at Newman's, in Lisle-street, and of many other philosophical instrument makers; and even the fabricators themselves of the culpable substitutes we have mentioned, might learn to make them properly, if they would only take the trouble to read the invaluable little volume before us. Perhaps they are not aware of its existence—it shall not be our fault, at all events, if they continue in their ignorance.

The work begins by some general views of the author's progress in his researches on the safety lamp, and of the principles on which its security depends; and this section we most especially recommend to the careful perusal of the wire-workers aforesaid. The principles are so clearly explained, that they cannot misunderstand them, and if they wilfully hereafter neglect them, they are answerable for all the dreadful consequences their inattention and folly may give rise to. We trust that the manufacturers of the lamps for the coal mines are not liable to the preceding censure; but even in the cellars of this gas-lighted metropolis, some serious accidents have happened, and their frequency may be much augmented if persons enter them in unguarded confidence in the false security of one of the lanthorns we have mentioned. That the said wire-workers may have still less ground to plead ignorance of the principle of the real safety lamp, we shall quote the most important passages on the subject in our author's own words.

Sir Humphry Davy began his experiments by a chemical examination of the substance with which he had to contend—the fire-damp of the coal mines. He found that it requires very large quantities of atmospheric air to produce an explosive mixture; namely, about seven or eight times its volume of air, in which proportion it explodes with most energy—with between five and six volumes it explodes feebly; and it retains its explosive power, when the proportions are one volume of gas to fourteen of air.

He found the fire-damp much less combustile than other

inflammable gases : red-hot iron or charcoal do not explode it; but iron white-hot, and in brilliant combustion,\* inflames it. .

One part of carbonic acid, mixed with seven parts of an explosive mixture, or one part of nitrogen with six parts, destroys its explosive power.

“ In exploding a mixture in a glass tube of one-fourth of an inch in diameter, and a foot long, more than a second was required before the flame reached from one end to the other, and I found that in tubes of one-seventh of an inch in diameter, explosive mixtures could not be fired when they were opened in the atmosphere; and that metallic tubes prevented explosion better than glass tubes.”

Reasoning on these curious and important phenomena, Sir Humphry Davy was not long in perceiving the principles that guided him in his subsequent investigations. He saw that “as a *considerable* heat was required for the inflammation of the fire-damp, and as it produced in burning comparatively a *small degree* of heat, the effect of carbonic acid and azote, and of the surfaces of small tubes in preventing its explosion, depended upon *their cooling powers*; upon *their lowering the temperature of the exploding mixture so much, that it was no longer sufficient for its continuous inflammation* ;” and this idea, so ably and readily grasped by the powerful mind of our author, “ led to an immediate result—the possibility of constructing a lamp, in which the cooling powers of azote or carbonic acid formed by combustion, or *the cooling powers of the apertures* through which the air entered or made its exit, should prevent the communication of explosion.”

Our limits will not allow us to detail at length all the steps by which Sir Humphry Davy brought his lamp to the perfection it ultimately possessed; we can only briefly mention that his first trials were on the effect of lamps supplied with a very limited circulation of air; then on lamps furnished with systems of tubes at the top and bottom, and of such diameters that explosions could not pass through them; but he found that unless the tubes were very short and numerous, the flame could not be well supported; “ and in trying tubes of the diameter of one-seventh or one-eighth of an inch, I determined that they were safe only to small quantities of explosive mixture, and when of a given length; and that tubes, even of a much smaller diameter, communicated explosion from a close vessel.”

Pursuing his experiments, Sir Humphry discovered that “ a *few apertures* even of very small diameter were not safe unless their sides were very deep; that a single tube of one twenty-eighth of an inch in diameter, and two inches long, suffered the explosion to pass through it; and that a *great number* of small tubes, or of apertures, stopped explosion even when the depth of their sides was only equal to their diameter; and at

last I arrived at the conclusion that a *metallic tissue*, however thin and fine, of which the apertures filled more space than the cooling surface, so as to be permeable to air and light, offered a perfect barrier to explosion; from the force being divided between, and the heat communicated to, an immense number of surfaces."

"My first safety lamps constructed on these principles, gave light in explosive mixtures containing a great excess of air, but became extinguished in explosive mixtures in which the fire-damp was in sufficient quantity to absorb the whole of the oxygen of the air, so that such mixtures never burnt continuously at the air-feeders, which in lamps of this construction was important, as the increase of heat, where there was only a small cooling surface, would have altered the conditions of security."

After many other attempts to construct a safety lamp which should answer in all mixtures of fire-damp, Sir H. Davy hit upon the simple and effectual expedient of surrounding the light entirely with wire gauze "and making the same tissue feed the flame with air and emit light."

"In plunging a light surrounded by a cylinder of fine wire gauze into an explosive mixture, I saw the whole cylinder become quietly and gradually filled with flame; the upper part of it soon appeared red hot, yet *no explosion* was produced."

Sir Humphry Davy next proceeded to put the efficacy of this admirable invention, which, whilst it "excluded the necessity of using glass, or any fusible or brittle substance in the lamp, not only deprived the fire-damp of its explosive powers, but rendered it an useful light;" to the most severe trials he could devise, in order to be absolutely certain of its safety under all possible circumstances; and he found "that iron wire gauze, composed of wires from one-fortieth to one-sixtieth of an inch in diameter, and containing twenty-eight wires, or 784 apertures to the inch, was safe" in the most explosive atmospheres composed of the gas produced by the distillation of coal (which always contains olefiant gas, an explosive mixture of which with atmospheric air inflames by contact with red hot iron or charcoal) and common air.

This mode of structure was consequently adopted; and in January, 1816, the safety lamps were introduced in the coal mines, and have ever since been in general use.

In 1817, in consequence of his researches on the nature of flame, Sir Humphry Davy made an important practical addition to the lamp, founded entirely on a new principle. He suspended a little cage of fine platina wire within the lamp, by means of which the lamp yields a light in atmospheres too much contaminated with fire-damp to be explosive; "a slow combination being effected by the heated platina, between the

elements of the gas and oxygen, which produces sufficient heat to keep the metals of low conducting powers and low capacity for heat permanently ignited whenever there is air enough to support life without suffering."

Sir Humphry Davy, to whom we are indebted for the first correct notions on the nature of flame, defines it to be "aëriform or gaseous matter, heated to such a degree as to be luminous; flames are conical, because the greatest heat is in the centre of the mass, and because heated air rapidly ascends through cooler air." The heat of flame is proportional to the rapidity of combination, and the density of the gases combining; thus it diminishes by rarefaction and increases by condensation. If a gaseous mixture require a high temperature for its combustion, it will be easily extinguished by rarefaction, or by cooling agencies; if it require a very low temperature only, it will burn in highly rarified air, or under considerable cooling agencies.

Gases that burn with difficulty, are easily kept in a state of continued inflammation if they be strongly heated; and the makers of safety lamps cannot be too sensibly impressed with the consequences which flow from this property, for "if mixtures of fire-damp are burnt from systems of tubes or canals, or metallic plates, which have small radiating and cooling surfaces, *though these systems are safe at first, they become dangerous as they are heated,\** where currents are occasioned which concentrate explosive mixtures, by the air feeders in lamps being below \* and made in thick metallic plates or canals, there being an increment of heat within, and a very small radiating surface without, as *the heat increases, the combustion of the explosive mixture will gradually extend further, and at last communicate with the external air, for explosion will be communicated by any aperture, however small, provided it be sufficiently heated.*"

"Wire gauze, as it offers a greater extent of radiating surface than perforated metallic plates, is the best material for the guard of lamps; and by being made of the proper degree of fineness, it will form a barrier for every species of explosion requiring temperatures higher than those of our atmosphere; but the apertures must be smaller, and the radiating surfaces greater, in proportion to the inflammability of the gas; and currents of explosive mixtures, acting even for a length of time, may be stopped by reduplications of wire gauze. Wire gauze for lamps must not be made of, nor covered with, any easily combustible metal; fine brass wire is improper, on account of the zinc it contains, and the iron wire should not be tinned.

\* "I warn the coal miner," says Sir Humphry "against any pretended safety lamps made in this manner, and which, to superficial observers, may appear to be constructed upon principles of security, but in which these principles cannot really exist."

The body of the lamp should be of copper riveted together, or of massy cast brass, or cast iron; the screws should fit tight; no aperture, *however small*, should be suffered to exist in the body of the lamp, and the trimming wire should move through a long tight tube. The temperature of metal, even when white hot, is far below that of flame; and hence red-hot gauze, in sufficient quantity, and of the proper degree of fineness, will abstract sufficient heat from the flame of the fire-damp, to extinguish it."

We have dwelt so copiously on the first section of this interesting book, from its great practical importance, that we can spare very little space for the others, which occupy by far the larger portion of the volume, and consist of extracts from papers published by the author in the Philosophical Transactions and the Journal of Science, on the Fire Damp, the Safety Lamp, and on Flame; a description of the plate at the beginning of the work—and some extracts from communications, on the application of the safety lamp, from Mr. Buddle, and other gentlemen practically connected with the coal mines, all giving the most satisfactory testimony of the merits and efficacy of this important discovery.

We shall quote one more important practical remark from the *conclusion*. The increased heat produced by an explosive mixture in rapid motion, requires that the radiating or cooling surface of the lamp should be increased, or the circulation of air diminished. For this purpose "twilled gauze, or a double or triple fold of wire gauze on one side of the lamp, or a screen of metal opposite to the direction of the current, or a semi-cylinder of glass or mica within, answers perfectly the object of preventing the heat from rising to redness."

A short Appendix is added in the present publication, containing five articles. The first states that the author has received a paper from M. Gay-Lussac, written some years ago by M. de Humboldt and himself, the results of which are confirmatory of Sir Humphry Davy's principles on the causes of combustion and explosion, and show that mixtures of oxygen and hydrogen in proportions not inflammable by the electric spark, may still be made to combine and form water by artificially raising their temperature.

The second article relates to the aphlogistic lamp, and the recent experiments of Döbereiner and others on the effect of spongy platina to promote the union of oxygen and hydrogen gases, respecting which Sir Humphry Davy offers the following observations:

"A *probable* explanation of the phenomenon may, I think, be founded upon the electrochemical hypothesis, which I laid before the Royal Society in 1806, and which has been since adopted

and explained, according to their own ideas, by different philosophers."

"Supposing oxygene and hydrogen to be in the relations of negative and positive, it is necessary to effect their combination that their electricities should be brought into equilibrium or discharged. This is done by the electrical spark or flame, which offers a conducting medium for this purpose, or by raising them to a temperature in which they become themselves conductors. Now platinum, palladium, and iridium, are bodies very slightly positive with respect to oxygene; and though good conductors of electricity, they are bad conductors and radiators of heat, and supposing them in exceedingly small masses, they offer to the gases the conducting medium necessary for carrying off, and bringing into equilibrium their electricity without any interfering energy, and accumulate the heat produced by this equilibrium. Other metals do not possess the same union of qualities, yet most of them assist combination at lower temperatures than glass, which is a non-conductor of electricity."

"That spongy platinum, even when moistened, as M. Döbereiner has very lately shown, should facilitate the combination of oxygene and hydrogen, *may* depend upon *this peculiar* electrical property; and why foil of platinum should have its power of causing oxygene and hydrogen to combine, increased by being placed, for a short time, in nitric acid, as MM. Dulong and Thenard have shown, may be owing to this, that the slight positive charge it acquires may, in being brought into equilibrium, be a first step in the operation; and there are analogous instances."

"Fine wire of platinum, I find, when conveying currents of electricity, as in a circuit with zinc and sulphuric acid, or charcoal and nitro-muriatic acid, has not its power of acting upon gaseous mixtures sensibly increased."

No. 3 relates an explosion of inflammable gas, which in general is only disengaged in coal mines, that took place in 1818 at the Salt Works at Aussee, by which several persons were killed, in consequence of which the safety lamp has been introduced in the mines of Styria, Salzburg, and Upper Austria. The inflammable gas appeared to be derived from bituminous schist.

We copy the Articles No. 4 and 5 verbatim.

"No. 4. I have had some correspondence with Mr. Buddle respecting the accidents which have happened in coal mines since the discovery of the safety lamp. He refers them in all cases to the carelessness of workmen.

"I should strongly recommend double lamps in cases where miners are obliged to work for any time in explosive mixtures, or wherever currents are expected;—or lamps with mica, or tin-plate *within* the wire gauze to prevent too great a circulation

of air. It is very easy to extinguish a lamp in which the fire-damp is burning, by sliding a tin-plate cylinder over it, or by a circle of wire gauze fitting the interior in a rim of copper, and moved by the termination of the trimming wire; but it is much better, in all cases of danger, to use lamps which *under no circumstances* can explode. Such as those described in p. 97.

"Having often trusted my life to the safety-lamp under the most dangerous circumstances, I cannot but sometimes smile when the public papers endeavour to invalidate its security upon the opinions or evidence of certain persons who have their own nostrums for preventing the accumulation of inflammable air in mines.

"I have sometimes to read letters on the improvement of the invention by plans, most of which are discussed in the foregoing pages; such as using glass or mica as a part of the surface for transmitting light, using double lamps, or double lamps containing a reflecting surface to prevent explosions from currents; and I have actually seen a lamp upon the rudest model of those I first made, having thick glass above, and wire gauze below, called 'the newly invented safety lamp!'

"No. 5. For gas manufactories or houses where gas is extensively used, I should recommend the safety lamp with iron wire gauze; but for the use of the navy, those with copper wire gauze are less liable to rust. As the latest instance of a ship lost for want of a safety lamp, I may mention the Kent East Indiaman, which was burnt, as I am informed by the Shipping Committee, in consequence of the inflammation of rum, by means of a common lantern."

We cannot conclude our remarks on this subject without expressing our surprise and regret, that it has not been taken up by Parliament in the manner it deserves. If a nation's gratitude be due to her heroes and defenders, it is not less so to those who promote her internal resources and welfare; and in a moral point of view, the philosopher whose happy application of science preserves the lives of his fellow creatures, is even more entitled to it than the warrior who destroys them. We hope those who have the power to confer the reward will not, late as it is, altogether neglect what we cannot but feel is as imperious a duty, as we trust they will find it a grateful one.

## ARTICLE X.

### *Proceedings of Philosophical Societies.*

#### ROYAL SOCIETY.

April 28.—Capt. E. Home, R.N. was admitted a Fellow of the Society; and the reading of Dr. Granville's Monograph on

Egyptian Mummies was concluded: we are enabled to state shortly the principal object of this interesting communication, and to allude to some of the curious facts it details on the subject of embalming.

It appears, that in the year 1821, Sir A. Edmonstone presented Dr. Granville with a mummy he had brought from Upper Egypt, which, after the removal of innumerable bandages, proved to be that of a female, and a more perfect specimen of the kind than any that had heretofore been examined. Dr. Granville deemed this an excellent opportunity of investigating the yet unsettled question of the mode of embalming by the ancient Egyptians; and proceeded to dissect the mummy for that purpose in the presence of several medical and scientific friends; instituting, at a more recent period, several experiments on its various parts and envelopes, tending to discover the process of mummification, in which object he appears certainly to have succeeded.

This discovery he endeavoured to prove to the satisfaction of the persons present at the reading of his communication, synthetically as well as analytically; for after the meeting, an exhibition of the dissected mummy took place in the library of the Society, where every assertion contained in the paper was illustrated by preparations, including several specimens of imitative mummies prepared by the author, some of which bore the closest resemblance to the Egyptian, and had withstood putrefaction for upwards of three years, though exposed to all the vicissitudes of a variable climate without any covering or other precautionary measure.

Independently of this, which is evidently the main object of Dr. Granville's researches, the author has been able to advance many very curious facts connected with the mummy in question. He has, for instance, given the dimensions of its various parts, which, by a singular coincidence, happen to be precisely those assigned by Camper and Winkelmann to the celebrated statue of the Medicean Venus, the prototype of ideal beauty. These dimensions, moreover, prove, that this Egyptian female did not belong to the Ethiopian race, thereby contradicting the assertion of some writers, who consider the ancient Egyptians to have been Ethiopians. He has also fairly made out the age at which the individual died; and the disease of which she died; and he has rendered it evident, from anatomical demonstration, that she had borne children.

All these circumstances may be considered by some as possessing no interest; but when it is considered that they are deduced from a minute and accurate examination of the body of a female, who, according to the best authorities of the present day with respect to Egyptian antiquities, and judging of the excavation out of which the mummy was taken, must have

lived about 3000 years ago; it will be admitted, that the preserving power of the mummifying process employed by the ancient Egyptians, and now discovered by Dr. Granville, must be great indeed.

This mummifying process consists in the thorough impregnation of every part, soft or hard, with bees'-wax. There are besides, myrrh, gum, resin, bitumen, and even tannin (another new fact brought to light by the author of this paper) in the composition of the mummy; but none of these, either singly or conjointly, appear to be sufficient without the bees'-wax, to preserve the body, or convert it into a perfect mummy. Dr. Granville has proved this by successive steps, and convinced those who saw the exhibition after the meeting of its accuracy, by showing one of the *nates* of the mummy wholly deprived of the wax by ebullition and maceration, which was beginning to putrefy, and which now looked no longer like its mummified fellow, but more like the preparation of a recent specimen of that part.

The disease of which the female died was ovarian dropsy; and the uterine system, with the sac that had contained the morbid fluid during life, forming the oldest pathological preparation of its kind in existence, was exhibited to the society. The heart, lungs, diaphragm, one of the kidneys with the ureter, the gall bladder, and part of the intestines, were also shown.

*May 5.*—Dr. H. H. Southey was admitted a Fellow of the Society; and a paper was communicated by Peter Barlow, Esq. FRS., in a letter to Mr. Herschel, On the Magnetism imparted to Iron Bodies by Rotation.

*May 12.*—John Taylor, Esq. was admitted a Fellow of the Society; and a paper was read, On the Magnetism produced in an Iron Plate, by Rotation; by S. H. Christie, Esq. AM. FRS.

*May 19.* Mr. George Harvey, John Smirnov, Esq., and the Rev. Dr. Morrison, DD. were respectively admitted Fellows of the Society; and the following papers were read:—

A Description of the Transit Instrument by Dollond, erected at the Observatory at Cambridge; by Robert Woodhouse, AM. FRS.

On the Fossil Elk of Ireland; by Thomas Weaver, MRIA., &c.: communicated by Professor Buckland.

During his recent avocations in the North of Ireland, Mr. Weaver had enjoyed, he stated, an opportunity of determining some facts showing that the Elk whose fossil remains so frequently occur in Ireland, lived and died in the countries where it is now found; and similar facts had been ascertained about the same time, in the West of Ireland, by the Very Rev. Archdeacon of Limerick; particulars of which had been communicated to the Royal Dublin Society, and would form, Mr. Weaver

hoped, a distinct publication on the subject: but he gave some account of them in the present paper, as they directly confirmed his own deductions. Mr. Weaver's researches were made in the county of Down, which presents hills of 300 or 400 feet in height, consisting of alternate beds of clay-slate and fine grained greywacke, traversed by many contemporaneous veins of calcareous spar and quartz, and also intersected by some true metalliferous rake veins. Between two of these hills, at about four miles distance from the town of Dundrum, was the bog of Kilmegan, in which the facts were observed. It appears to have been a lake, which has been gradually filled up by the growth and decay of successive races of aquatic plants, and the consequent formation of peat; but on account of the water still remaining, it had never been worked as a peat-bog until the present Marquis of Downshire drained it by means of a level. The peat was found to rest upon a bed of marl, from one to five feet in thickness, consisting of a calcareous base mingled with comminuted fragments of freshwater shells, which it likewise contained in an entire and but slightly altered state, all referable to three still existing species, viz. *Helix putris*, *L. Turbo fontinalis*, and *Tellina carnea*. Many bones and horns of the Elk had been found from time to time in this bog, all of which, Mr. Weaver ascertained, from the concurrent testimony of the tenantry, were found either between the peat and the marl, or slightly impressed in the latter.

The researches of the Archdeacon of Limerick had been made in a bog in that county: the bones were found under circumstances precisely similar, and upon marl of the same kind. From them the Archdeacon had been enabled, with the assistance of Mr. Hart, M.R.C.S. to frame a nearly complete and gigantic skeleton, which he had given to the Museum of the Royal Dublin Society. Some of the bones presented indications of disease; one leg had evidently been broken and healed again, a rib had a perforation about one-eighth of an inch wide, the edges of which were depressed on the outside, and raised on the inside; it was such as could only have been made by a thin sharp instrument, which did not penetrate far enough to cause a mortal wound; for, as the edges of the perforation were quite smooth, the animal must have survived the injury at least a twelvemonth. The bones seemed to retain all their principles, with the addition of a portion of carbonate of lime imbibed from the contiguous marl. Some of them still retained their marrow, which had the appearance of fresh suet, and blazed when applied to the flame of a candle. With them were found a pelvis, apparently belonging to a Red-Deer, and the skull of a Dog, of about the size of a Water-Spaniel.

From all these circumstances, which accord with those under

which the remains of the Elk occur in the currachs of the Isle of Man, as described by Mr. Henslow, Mr. Weaver infers that these Elks must have lived and died in the countries where they are now found; that the period when they lived must be considered as modern in the physical history of the globe; and that their destruction is to be attributed to the constant persecution of their enemies, and in some cases to the operation of local causes; and not to a catastrophe acting on the entire surface of the globe: thus, that their remains are not of *diluvial*, but of *post-diluvial* origin. Mr. Weaver conceives that they fled to the lakes, which have since become bogs, as a refuge from their enemies, and so often found a grave where they had sought protection.

## GEOLOGICAL SOCIETY.

Feb. 18.—A paper by Professor Buckland was read on the valley of Kingsclere near Newbury, and the evidence it affords of disturbances affecting the green sand, chalk, and plastic clay formations.

The object of this paper is to describe the phenomena of a small valley near Kingsclere, in which the green sand strata are protruded to the surface through the chalk and plastic clay, at a spot situated within the area of the chalk basin of Newbury, and affording a remarkable exception to the general regularity of that basin.

This irregularity of structure has apparently originated from a sudden elevation of the chalk, accompanied by fracture and an inverted dip; its position is remarkable as being near Inkpen hill, a point where the chalk rises to 1011 feet, the highest elevation it attains in England.

In the valley subjacent to the Inkpen ridge, and near its north base, the chalk dips rapidly in two opposite directions nearly N and S on each side of a central axis or anticlinal line; and a little further east the green sand also emerges with a similar double dip, and forms the small valley of Kingsclere, surrounded on all sides with an enclosing escarpment of chalk.

The N frontier of this valley is in close contact with well characterized deposits of plastic clay dipping like itself rapidly towards the north. Four similar valleys are adduced in the counties of Wilts and Dorset; and the author concludes respecting them all, that it is utterly impossible to explain their origin by denudation alone, nor indeed without referring the present position of their component strata to a force acting from below and elevating the strata along the line of the central axis of the valleys in question. To valleys of this kind the author applies the appellation of valleys of elevation, to distinguish them from those which owe their origin simply to *diluvial* denudation. He then proceeds to show, that the

valleys of Pewsey near Devizes, and of the Wily and the Nadder above Salisbury, have also, to a certain degree, been affected by a force acting from beneath, and elevating the strata at a period antecedent to their being submitted to denudation; and concludes, that not only these enclosed valleys similar to that of Kingsclere, but many open valleys also (though in all cases modified by subsequent denudation), had a prior origin arising from the fracture and elevation of their component strata: this must have happened in the case of the Weald of Kent and Sussex, enclosed as it is with an escarpment of chalk dipping every where outwards in opposite directions, and sometimes, very rapidly, along the North and South Downs.

The author proceeds to illustrate, by the position of the strata of plastic clay in this same district, the important Geological question whether the chalk was disposed in its present form of troughs or basins, before or after the deposition of the tertiary formations now enclosed in them, and to show that the present inclination of the strata along the S frontier of the basins of London and Hants took place since the deposition of the plastic, and probably also of the London clays; and that these two basins were once connected together across the now intermediate chalky strata of the downs of Hants, Wilts, and Dorset; since it appears that the plastic clay formation is so far from being limited to the lower levels of the present basins, that large residuary fragments of it still occur on the summits of the most elevated portions of chalk in these counties, e. g. on the summit of Inkpen near Newbury, and on that of Blackdown near Abbotsbury, as well as on the top of Chidbury and Beacon hills in the highest part of Salisbury plain. The strata that covered the intermediate spaces have probably been removed by diluvial denudation, and the destructible nature of their component materials would render them peculiarly liable to be swept away by the transit of violent currents of water. The wreck of the harder portions of the sandy strata thus destroyed, forms the sandstone blocks called grey weathers that lie loosely scattered on the naked surface of the chalk in all these counties, and of which Stonehenge is constructed. In lower levels, within the existing basins, these same strata have been less destroyed, in consequence of the greater protection from the ravages of diluvial denudation which their low position has afforded them.

The author concludes by referring to the occurrence of similar tertiary strata as well as of chalk and green sand on the summit of the Savoy Alps, nearly 10,000 feet above the level of the sea, where they seem to bear the same relation to the tertiary strata of the valleys of Italy, France, and Germany, that our trifling elevations of Inkpen, Blackdown, &c. bear to the basins of London and Hants, and concludes that

since the deposition of these beds, either by the elevation of the mountains, or the depression of the valleys, or the united effect of both these causes, the relative level of the one to the other has been changed to the amount of many thousand feet.

*March 4.*—A notice was read on some silicified wood from the desert between Cairo and Suez, in a letter by George Francis Grey, Esq. to the Rev. W. Buckland, Pres. GS.

Large masses of silicified wood, resembling in form the trunks of palm trees, lie scattered, the author observes, over a tract of gravel in the desert about fifteen miles from Cairo, and for two days' journey all the way from that place to Suez.

A notice was also read on the bones of several animals found in peat near Romsey, in Hampshire, extracted from a letter from Charles Daman, Esq. to the Rev. W. Buckland, Pres. GS.

Mr. Daman mentions that the skulls of several beavers, as well as the bones of oxen, swine, stags and roebucks, have been dug out of the peat near Romsey, and out of the shell marl provincially termed "malm," which occurs in the same alluvial tract. In one place several human skeletons have been taken out of the marl.

A paper entitled "Observations on the beds of clay, sand, and gravel belonging to the red marl formation of the midland counties, and on the rocks from which they are derived, by the Rev. James Yates, MGS." was read in part.

*March 18.*—The paper entitled "Observations on the beds of clay, sand, and gravel belonging to the red marl formation of the midland counties, and on the rocks from which they are derived, by the Rev. James Yates, MGS." was concluded.

In this communication Mr. Yates enters into some description of the rocks which are found *in situ* on the confines of Wales and Shropshire, in order to show, that from the disintegration of these rocks, the clay, sand, and gravel of the red marl formation have for the most part been derived. The first line of section which is particularly considered is near the river Dee and Valle Crucis; the second, a line drawn from Oswestry westward to Llansilen, which crosses within the space of five miles the baset edges of all the strata from the new red sandstone to the slate. The author then takes a view of the rocks occurring in the direction of the road from Welchpool to Ludlow. The fourth district then noticed is the vicinity of Church Stretton. Mr. Yates then mentions some particulars of the rock near Bewdley, and in the Clent hills, and the neighbourhood of Dudley, and adds some remarks on the Broomsgrove Lickey, as supplementary to Professor Buckland's paper in the fifth volume of the Geological Society's Transactions.

The range of hills is also described which extends from NW

to SE beside the course of the Coventry canal and the river Anker; and lastly, a district in Leicestershire, a few miles E from Ilkley, consisting of a coarsegrained crystalline greenstone.

The author then proceeds to show, how the strata belonging to the older formations, which he has described, may be viewed in connexion with the general physical structure of England, and then points out from what various sources the beds of sand, clay, and gravel of the red marl formation, as well as the superficial debris which is strewed over the midland districts of England, may have originated. Mr. Yates concludes with some remarks on the excavation of valleys, and on some opinions on that subject now generally received among English geologists, from which he is inclined to differ.

*April 15.*—A paper was read entitled “On a New Species of Gyrogonite from the lower freshwater formation at Whitecliff bay, in the Isle of Wight, with some account of the strata in which it occurs.” By Charles Lyell, Esq. Sec. GS.

Mr. Lyell describes this species of gyrogonite as very distinct from the three species which have been found in France. The spiral valves form nine rings, each of which are ornamented with a row of tubercles, from which he has given it the name of *chara tuberculata*. An account is given of the strata of the lower freshwater formation at Whitecliff bay in the Isle of Wight, in which this gyrogonite occurs very abundantly. They consist of beds of very compact limestone, alternating with whitish calcareous marls, and in most of them the casts or shells of various freshwater univalves are common.

Gyrogonites appear not to have been noticed before in the freshwater strata on the east side of the Isle of Wight. Those which have been noticed as abounding in the limestone of the lower freshwater strata at Garnet Bay are chiefly referable to the *chara medicaginula* of the French authors. In that locality, fossil stems accompany them whose structure is identical with that of some recent *charæ*, as for example *C. hispida*.

The author concludes by observing, that from the remarkable toughness of the integument of their seedvessel, and from the large proportion of carbonate of lime which they contain in a living state, most of the *charæ* are peculiarly adapted for becoming fossil, and that they are accordingly preserved in the recent marls in Scotland, both in a vegetable and a mineralized state, when the other aquatic plants which lived and died in the lakes with them are entirely decomposed, or can no longer be recognized.

An extract of a letter was read from Jer. Van Rensselaer, Esq. on the Discovery of the Skeleton of a Mastodon at New York; and of the Tertiary Formation in New Jersey.

In this letter Mr. Rensselaer mentions, that in a late expe-

dition which he had made with some friends to examine the geology of the state of New Jersey, they had discovered, disinterred, and afterwards brought to New York, the skeleton of a mastodon very nearly perfect. They also satisfied themselves that much of the region which lies between the Atlantic and the range of primitive mountains was referable to the tertiary formation, and that the secondary do not make their appearance for some hundreds of miles.

A paper was read entitled "Account of a Fossil Crocodile recently discovered in the Alum Shale near Whitby." By the Rev. George Young.

Mr. Young describes the osteology of this fossil animal, which has been deposited in the museum at Whitby, and of which a drawing accompanied this communication. Its length exceeds fourteen feet, and when perfect must have reached eighteen.

The author mentions that these are not the only remains of the crocodile which have been discovered near Whitby, although they had been generally confounded with those of the plesiosaurus; of which animal, however, as well as of three or four species of the ichthyosaurus, undoubted remains occur in the Alum Shale of Whitby.

## ARTICLE XI.

### SCIENTIFIC NOTICES.

#### MISCELLANEOUS.

##### 1. *New Scientific Journal.*

In spite of the old adage, that two of a trade can never agree, (which by the bye, for the sake of human nature, we hope is not more true than it is liberal,) we have much pleasure in announcing the appearance of the first number of the Dublin Philosophical Journal and Scientific Review, and in bearing our testimony to its merits. If it be carried on in future with as much ability as is shown at its outset, it will prove a valuable addition to the scientific journals of the day, and reflect great credit on the zeal of its editors and the talents of our fellow labourers in the sister kingdom. The present number contains many original and valuable articles, particularly one by Dr. Brinkley, which opens the work, *On the Method of finding the Longitude from the Culmination of the Moon and Stars*; a paper by Mr. Lloyd on the *Composition of Forces*; another on the *Crystallization of Precipitates*, by Mr. Stokes; one by Dr. Jacob, on the *Generic Characters and Anatomical*

*Structure of the Whale*; a description of an ingenious *Apparatus for filtering out of Contact with the Atmosphere*, by Mr. Donovan, and several others. In the *Review* department, we have particularly to notice an excellent account of Mr. Daniell's *Meteorological Essays*, in which, though the critic does not always agree with his author, much well merited praise is bestowed on that very interesting volume. The review of a book called *The Young Brewer's Monitor*, is pretty severe (not unjustly so, however, as it should seem) and very entertaining, and a good specimen of the *ridiculum acri*, as well as the subsequent one on *Brown's Principles of the Differential Calculus*. In our next number we intend to make our readers more intimately acquainted with some of the articles in this promising journal.

## 2. *New Magnetic Phenomenon.*

At the sitting of the Royal Academy of Sciences of Paris, on the 7th of March, M. Arago exhibited an apparatus for showing, in a new form, the action which magnetized and non-magnetized bodies mutually exert on each other.

In his first experiments, M. Arago proved, that a disc of copper, or any other solid or liquid substance, placed beneath a magnetic needle, affects the extent of its oscillations, without sensibly altering their duration. The phenomenon in question may be considered as the converse of the preceding. Since a needle in motion is stopped by a disc at rest, M. Arago imagined that a needle at rest would be moved by a disc in motion. In fact, if a plate, of copper for instance, be made to turn with any determinate velocity under a magnetized needle contained in a perfectly closed vessel, the needle will no longer assume its usual position; it stops without the magnetic meridian, and so much the farther from that plane as the revolution of the disc is more rapid. If the rotatory motion be sufficiently rapid, the needle itself, at whatever distance from the disc, turns continually round the wire on which it is suspended.—(*Annales de Chimie*.)

## 3. *Hyæna Caves in Devonshire.*

Professor Buckland has lately examined two caves in Devonshire, in both of which he found, in a bed of mud beneath a crust of calosinter, gnawed fragments and splinters of bones, with teeth of hyænas and bears. There were no entire bones, except the solid ones of the toes, heels, &c., as at Kirkdale, which were too hard for the teeth of the hyæna. They appear simply to have been dens, but less abundantly inhabited than that at Kirkdale. In the same cave, Professor Buckland found one tooth of the rhinoceros, and two or three only of the horse.—(*Edin. Phil. Journ.*)

4. *Quantity of Blood in Animals.*

Those who have not considered the subject, must be surprised at the quantity of blood which passes through the heart of any moderately sized animal in the course of 24 hours. In man, the quantity of blood existing in the body at any given moment is probably from 30 to 40 pints. Of these, an ounce and a half, or about three table spoonfuls, are sent out at every stroke; which multiplied into 75 (the average rate of the pulse), give 1125 ounces, or seven pints, in a minute; i. e. 420 pints, or 25.5 gallons, in an hour; and 1260 gallons, i. e. nearly 24 hogsheads, in a day. Now, if we recollect that the whale is said to send out from its heart at each stroke 15 gallons, the imagination is overwhelmed with the aggregate of the quantity that must pass through the heart of that animal in 24 hours. It is a general law, that the pulse of the larger animals is slower than that of the smaller; but even if we put the pulse of the whale so low as 20 in the minute, the quantity circulated through the heart, calculated at 15 gallons for each pulsation, will be 432,000 gallons, equal to 8000 hogsheads in 24 hours. The consideration of this amazing quantity is, however, a subject of mere empty wonder, if not accompanied with the reflection, that, in order to produce the aggregate amount, the heart is kept in constant motion; and that, in fact, it is incessantly beating, as it is termed, or throwing out the blood into the arteries, from the first period of our existence to the moment of our death, without any sensation of fatigue, or even without our consciousness, excepting under occasional corporeal or mental agitation.—(Dr. Kidd, Edin. Phil. Journ.)

## ARTICLE XII.

## NEW SCIENTIFIC BOOKS.

## PREPARING FOR PUBLICATION.

Mathematical Tables. By W. Galbraith.

Flora Fossilis, or a Description of the Fossil Vegetable Remains found in the Coal Districts of Durham and Northumberland, with a particular Account of the concomitant Stratification. By J. B. Taylor, FSA.

Narrative of a Journey into Khorasan, with some Account of the North-east of Persia. By J. B. Fraser.

A Complete History of the Cistus or Rock Rose. By Mr. Sweet.

## JUST PUBLISHED.

Dendrologia Britannica, or Trees and Shrubs that will live in the open Air of Britain throughout the Year. By P. W. Watson, FLS. 2 vols. royal 8vo.; 172 coloured Plates. 5l. 5s.

The Study of Medicine. By J. M. Good, MD. Second Edition, enlarged and remodelled. 5 vols. 8vo. 3*l.* 15*s.*

Excursions to Madeira and Porto Santo, during the Autumn of 1823. By the late Edward Bowdich, with an Appendix, containing Zoological and Botanical Descriptions. Plates, 4to.

A Series of Tables, in which the Weights and Measures of France are reduced to the English Standard. By the late Christopher Knight Sanders. 8vo. 7*s.* 6*d.* boards, or 8*s.* 6*d.* half-bound.

A Treatise on Mineralogy, translated from the German of Frederic Mohs. By W. Haidinger, FRSE. 3 vols. post 8vo. 1*l.* 16*s.*

Narrative of a Journey across the Cordilleras of the Andes, and of a Residence in Lima, &c. By Robert Proctor. 8vo. 12*s.*

Voyage of Discovery in the Interior of Africa, from its Western Coast to the Niger. By Brevet Major Gray, 8vo. Plates and Maps. 18*s.*

The Surgical Anatomy of the Arteries of the Human Body. By Robert Harrison, AM. &c. Vol. II. 5*s.*

## ARTICLE XIII.

### NEW PATENTS.

R. Roberts, Manchester, civil engineer, for improvements in the mule, billy, jenny, stretching frame, or other machines used in spinning cotton, wool, or other fibrous substances, and in which either the spindles recede from and approach the rollers or other deliverers of the said fibrous substances, or in which such rollers or deliverers recede from and approach the spindles.—March 29.

J. H. Baker, Antigua, now residing in St. Martin's-lane, for improvements in dyeing and calico-printing by the use of certain vegetable materials.—March 29.

Maurice de Jongh, Warrington, cotton spinner, for improvements in spinning machines and mules, jennies, slubbers, &c.—March 29.

E. Sheppard, clothier, and A. Flint, Uley, Gloucestershire, engineer, for improvements in machinery for raising the wool or pile on woollen or other cloths by points, also applicable to brushing, smoothing, and dressing cloths.—March 29.

T. Parkin, Bache's-row, City-road, merchant, for a mode of paving parts of public roads, whereby the draft of waggons, carts, coaches, and other carriages, is facilitated.—March 29.

R. Cabanel, Melina-place, Westminster-road, engineer, for improvements on engines or machinery for raising water, part of which machinery is applicable to other useful purposes.—March 30.

J. Heathcoat, Tiverton, lace-manufacturer, for improved methods of figuring or ornamenting various goods manufactured from silk, cotton, flax, &c.—March 31.

J. J. Fisher, Ealing, Middlesex, for a new application of rail-ways, and the machinery to be employed thereon.—April 2.

S. Broadmeadow, Abergavenny, civil engineer, for his apparatus for exhausting, condensing, or propelling air, smoke, gas, &c.—April 2.

## ARTICLE XIV.

## METEOROLOGICAL TABLE.

1825.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.
		Max.	Min.	Max.	Min.		
4th Mon.							
April 1	N E	30.61	30.61	52	25	—	
2	N E	30.61	30.51	59	32	—	
3	S W	30.51	30.40	62	35	—	—
4	N	30.42	30.40	62	30	—	
5	N E	30.48	30.42	62	38	—	
6	E	30.49	30.48	55	25	—	
7	E	30.51	30.49	55	35	—	
8	E	30.51	30.39	58	28	—	
9	N E	30.40	30.39	68	34	.95	
10	N E	30.40	30.30	69	44	—	
11	N W	30.30	30.20	69	44	—	
12	W	30.22	30.20	60	44	.44	—
13	W	30.26	30.22	54	40	—	—
14	W	30.30	30.26	64	44	—	
15	W	30.30	30.27	64	40	—	
16	N W	30.36	30.27	64	40	—	
17	N W	30.36	30.36	55	29	—	
18	N	30.36	30.36	54	26	.84	
19	N	30.36	30.31	58	35	—	
20	W	30.31	30.15	58	36	—	
21	S W	30.15	29.82	58	36	—	12
22	S W	29.82	29.66	60	46	—	17
23	S W	29.66	29.60	66	49	—	19
24	S	29.82	29.60	66	40	—	40
25	S E	29.82	29.79	65	40	—	02
26	E	29.79	29.48	66	48	—	53
27	E	29.52	29.45	61	39	—	10
28	SE	29.64	29.52	62	38	—	02
29	SE	29.88	29.64	66	42	.97	
30	S	29.89	29.88	63	43	.14	
		30.61	29.45	69	25	3.34	1.55

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

## REMARKS.

*Fourth Month.*—1—8. Fine. 9. Foggy morning: very fine day. 9—11. Fine. 13. A little gentle rain this morning. 14—21. Fine. 22. A gentle rain this morning: showery day. 23. Fine. 24. Showery afternoon. 25. Fine: some thunder this afternoon. 26. Thunder at intervals during the day, with showers of rain and hail. 27. Showery. 28—30. Fine.

## RESULTS.

**Winds:** N, 3; NE, 5; E, 5; SE, 3; S, 2; SW, 4; W, 5; NW, 3.

**Barometer:** Mean height

For the month. ....	30.158 inches.
For the lunar period, ending the 10th. ....	30.414
For 14 days, ending the 1st (moon north) ....	30.375
For 13 days, ending the 14th (moon south) ....	30.387
For 14 days, ending the 28th (moon north) ....	29.961 ..

**Thermometer:** Mean height

For the month. ....	49.33°
For the lunar period, ending the 11th. ....	41.30
For 31 days, the sun in Aries. ....	45.467

Evaporation. ....	3.54 in.
Rain. ....	1.55
And by a second gauge. ....	1.61

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